PROCEEDINGS
OF THE
AMERICAN PHILOSOPHICAL SOCIETY
HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

VOL. L MAY–AUGUST, 1911 No. 199

NOTES ON CANNON—FOURTEENTH AND FIFTEENTH CENTURIES.

BY CHARLES E. DANA.

(*Read April 20, 1911.*)

There can be seen to-day in the fair city of Florence, on the Arno, an old yellow parchment, upon which is transcribed an edict dated February 11, 1326. This, expressed in the monkish Latin of the day, gives authority to the Gonfalonier, the Priors, and twelve "good men," to superintend the manufacture of "palloctas ferras et canones de metallo," balls of iron and cannon of metal, which may possibly, in this case mean *brass*. What these cannon for the defence of Florence were like, or what they did, we shall never know, but with them the real history of ordnance begins; these little pop-guns are the ancestors of the 14- and 15-inch B. L. R. (breech-loading rifle) of today; the fathers threw a wee projectile a hundred or two yards; the degenerate offspring throw a shell weighing about a ton, fifteen or twenty miles. That the Florentine guns were the very first no one would assert, but with our present information, only legend lies back of them.

Of course the credit for the invention is given to the Chinese. There is not time here to do more than state that the *Institutes of Timur,* about the middle of the fourteenth century, although they

PROC. AMER. PHIL. SOC., L. 199 J, PRINTED JUNE 26, 1911.

Cannon of 1390–1400.

One of the earliest representations of a Fire-arm. (From German Codex. Royal Library, Munich.)

give full details of the equipment of his troops, do not mention either cannon or gunpowder. The *"Wuh-li-Siao,"* published 1630, says "gunpowder came from the outer barbarians."

The mention of an explosive in the *Sukranita,* a Hindu work said to ante-date everything Chinese, is admitted by experts, I understand, to be a modern interpolation.

The "Liber Ignium a Marco Graeco Descriptus," dating back of the eleventh century, gives some 22 to 35 recipes for the so-called "Greek-Fire" etc. No Greek or Moslem writer ever uses the term "Greek-Fire." Col. Hime, an authority on this subject[1] concludes that the earlier recipes in the "Liber Ignium," were translated from the Arabic by a Spaniard. The first four recipes are for the compounding of "sea-fire," or, as there described, mixtures which will ignite "when rain falls on them." Quicklime was the cause of ignition; to it was added (C. 1300) sulfur, oil, gum Arabic; (C. 1350) sulfur and turpentine; (C. 1405) sulfur, petroleum, wax. None of these were true explosives.

Berthold Schwartz, of Freiburg, in Breisgau, the favorite German discoverer of gunpowder, made his discovery about 1320 to 1330, at the time the Florentines were popping off their "canones de metallo." Schwartz is said to have preceded the Florentines in the making of cannon but this claim has not as yet been established.

Lieut. Col. Hime undertakes to translate the "Epistola de secretis," of the liberal minded Friar Bacon (1214?–1294). This letter is probably earlier than 1249. It is written according to some cryptic method, a bad habit both famous Bacons indulged in, and if the secret of the over-cautious Friar has been guessed with even partial success, we have a right to suppose that while the good Friar was "experimenting with some incendiary compositions . . . the mixture exploded and shattered all the chemical apparatus near it" (Hime. 161). After this smash-up, Bacon could not fail to be convinced that saltpeter, sulfur and charcoal, when mixed in right proportions, had a distinctly explosive tendency—but he never seems to have advanced the next step and discovered the projectile force of the compound.

[1] Lieut.-Col. Henry W. L. Hime, "Gunpowder and Ammunition," London, 1904.

A few more words on gunpowder. In early days saltpeter was most difficult to procure; it was collected from cellars and caves; later, depots were established for its reception, while strict laws were passed to ensure its purification and baking. Costing much, saltpeter was very sparingly used, much to the detriment of the gunpowder of course. The proportions differ greatly according to the kinds of powder—whether, for cannon, priming, hand gun, etc., from equal parts of each; to, saltpeter, 3, sulfur, 1 ½, charcoal, 1 ½ to 4: 1: 1. and 6: 2: 1. The formula of today being about 6: 1: 1.

The price of gunpowder in the fourteenth century seems to have been almost prohibitive. Assuming that my figures are correct, which is more than doubtful, for there is no real standard of value, the price was, in money of to-day, rarely as low as twenty-five dollars, and quite possibly, occasionally, as high as fifty dollars a pound; now it costs a quarter of a dollar or less a pound. These prices rapidly decreased with the systematic collecting of saltpeter.

In a campaign the ingredients for making powder were carried separately, and mixed only when need came. Here is a note or two from an authority of about 1465. Keep the three ingredients separate, as the niter and sulfur if mixed soon spoil. Better carry the willow wood unburned, as charcoal absorbs the damp. A secret process for the preservation of powder: Take clear and very strong vinegar, make the powder into a paste; form cakes of four to eight livres (*livre*, about a pound), dry in the shade, sun, or even in an oven. We are getting close to granulated powder. As the usual powder was in the form of a very fine dust, the ignition must have been slow, and much of it was, in all probability, blown out at the muzzle.

The next mention of cannon is in an "indenture" of 1338, between John Starylyng, former keeper of the "King's vessels" (Edward III.) and Hemyng Leget. "Ij [ij] canons de ferr sanz estuff," presumably, without ammunition. Also "un canon de ferr ove ii chambres, un autre de bras ove une chambre." The cannon with two chambers was the form of breech-loader often used even for large bombards until the early part of the next century, and for smaller iron and brass cannon until the art of casting iron guns was well understood (in England not until c. 1545), and even into the

Loading Cannon of 1390–1400.

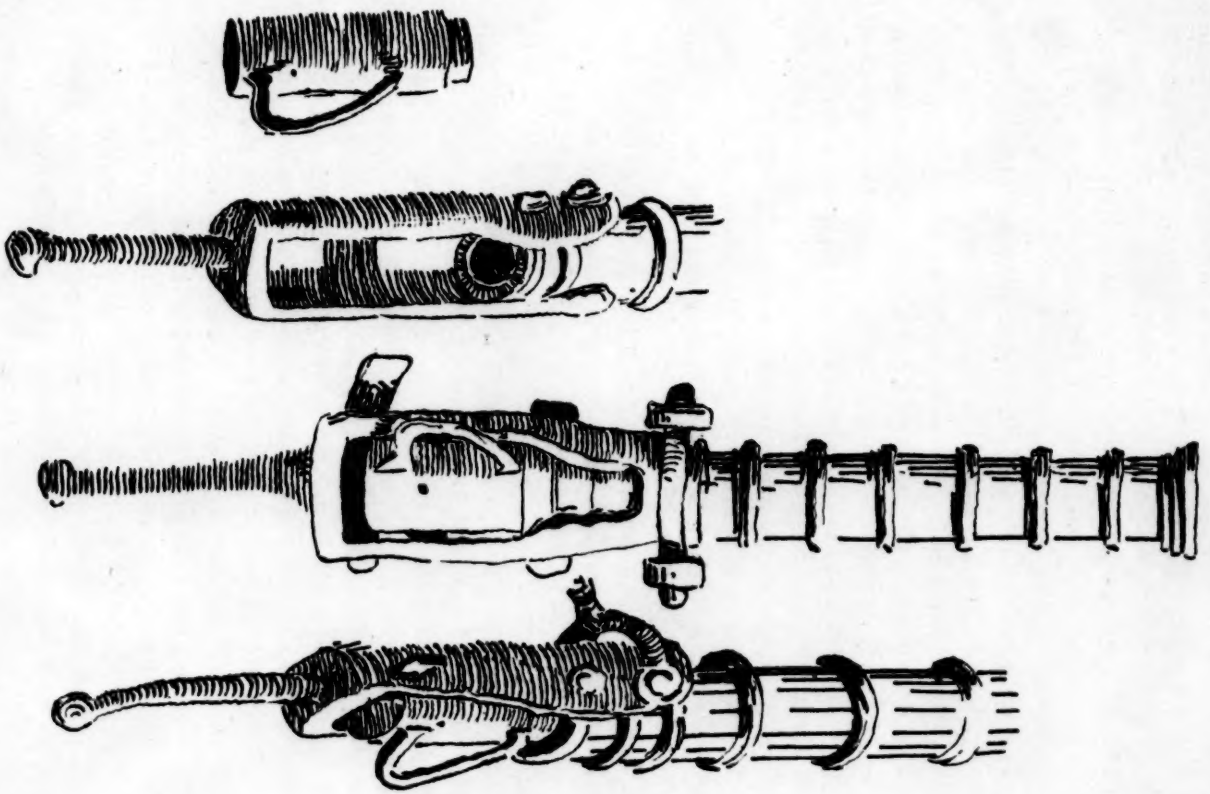
Cutting off wooden Plug for Wad. (From German Codex. Royal Library, Munich.)

seventeenth century, especially for guns used in China. An iron tube was fastened by bands of iron to a strong stock, with a space hollowed out at the breech to hold an iron box, the "chamber," sometimes called "a pot." Later this hollow became part of the gun, a sort of basket, or cradle. The chamber contained the powder charge, upon which a round section of wood was tightly driven as a wad. The projectile was placed at the breech end of the tube, with a straw, felt or rag wad in front to hold it; the chamber was wedged against it, then primed, and touched off with a heated iron rod. Needless to say that a goodly portion of gas escaped between chamber and tube. These were the earliest quick-firing guns, and in the sixteenth century were used on the upper works of the ships, very much as we use quicker firing guns today; the ancient ones, when there were four chambers, say, could be discharged about every two minutes. Later they will be called "murtherers"; and well they earned the name when the projectiles consisted of rusty nails, bullets and scraps.

The chambers, or "canones," for the huge bombards, which we shall meet with later, were held in place by heavy timbers. By or before 1400 the heavy, wrought-iron powder chamber was welded or screwed onto the chase of the bombard.

In the year 1338 appeared, in the Arsenal of Rouen, a terrible engine of destruction, called by its proud keepers, a "pot de fer à traire garros," an iron pot for throwing arrows. These arrows, much like cross-bow bolts, were tipped with iron and winged with brass, the latter metal obtained from kitchen utensils, cut up and melted for the purpose. The projectile was wound with leather to make it fit snug in the barrel. The powder charge for this dread engine of war was about seven tenths of an ounce of the ill-proportioned powder of that day. When all was prepared, and fire was applied, the bolt of destruction no doubt emerged, but certainly with considerable reluctance.

A recipe of a few years later enables us to approximately figure out the cost of cannon of that day. Five cannon of wrought-iron weighing 25 lbs. each, and five "canon de metal," presumably brass, cost three hundred dollars of to-day, say $30 each. All of which is submitted with considerable hesitation.



Chamber-Piece, 1338–c. 1700.

Chamber or "Pot," to hold powder charge. Cradle and Breech of Chase. Tail for pointing. Chamber wedged in place; part of Chase removed to show Projectile. Side-view showing means of horizontal and vertical motion.

The first mention of cannon by Froissart, who is as "faithful as an eye witness," is in the year 1340 at the siege of Quesnoy, on the northeast border of France, not very far from Valenciennes. "Those of Quesnoy let them hear their cannons and bombards, which flung large iron bolts in such a manner as made the French afraid of their horses."[2]

The earliest English use of this word *bombard* given by Dr. Murray, is in a quotation from John Lydgate, 1430. The noun has left us, but the verb, *to bombard* still lingers.

It is usually asserted that the first field-guns were used at the battle of Crécy, August 26, 1346. The Florentine chronicler, Giovanni Villani, remarks, in the somewhat florid manner of the time, "the bombards of the English made balls of iron to leap with fire, to frighten and drive off the horses of the French. . . . That the roar of the bombards made such a trembling of the earth, such a noise, that it seemed as if God thundered, with great slaughter of men and beating down of horses."

This terrible slaughter must have been produced by three small toys somewhat like blunderbusses, the charge for each of which was an ounce, more or less, of very bad powder. Cause and effect do seem disproportionate.

The "Grandes Chroniques de St. Denis" assert that it was the three cannons of the English that spread panic amongst the Genoese cross-bowmen and made them indulge in the singular antics by which they sought to frighten the English archers. In only one known copy of Froissart is there any mention of cannon at Crécy; this happens to be that in the library of the city of Amiens, not far from the battlefield; there is some reason to believe that the words in question are an interpolation; when one remembers that from Falkirk (1298) to Flodden (1513)—Bannockburn excepted—the English archer, firing ten or more armor-piercing projectiles a minute, with an effective range of 250 yards, was always victorious, it does appear possible that French writers, with more patriotism than truth, introduced these terrible cannon into their accounts of the battle as an excuse for the crushing disaster to their arms.

Edward III. took with him several cannon when he entered

[2] Chap. IV., Book I., p. 40.

France, July, 1346, the month before Crécy. We have too the King's Privy Wardrobe Accounts, as they were termed, giving lists of guns, ammunition, gunners and other details of the ordnance sent from the Tower of London to be used at the siege of Calais, which followed the battle of Crécy.

At this siege of Calais only leaden projectiles are mentioned, and from the very moderate amount of ammunition required for their propulsion, the guns although called "great" must have been exceeding small. The main reliance of besiegers and besieged continued to be in the huge engines for hurling masses of rock and other unpleasantnesses.

The derivation of the word *gun* is not without interest. Consulting both Murray and Skeat, we find that *Gunnhildr* was an Icelandic, female, proper name, once applied to war engines. As *Gunn* (Icel. *Gunnr*) signified *war,* and *hildr* a *battle,* it was certainly appropriate. An account of the munitions in Windsor Castle, 1300/01, mentions a large ballista named "Domina Gunilda." As there does not seem to have been any great lady, famous or infamous, so called in the fifteenth century, this is quite probably a survival of the old Scandinavian name. The M. E. word *gunne,* is, of course, but a shortened pet name for the fearsome lady.

Here is one of the early tragedies connected with cannon. In 1346, the year of Crécy, Peter of Bruges had established a high reputation for the making of "connoiles." The word may come from "tonnoiles," which in its turn, may have come from "tuyaux de tonnoire," or tubes of thunder. In September of that year the consuls of the city of Tournay hearing that connoiles were useful to be let off in a good town when besieged, desired the aforesaid Peter to make them one as a sample, and if it proved satisfactory they would give him an order for more. Peter, the thrifty burgher, did make one and then proceeded to show the worthy consuls what it would do. The connoile was placed with great care, outside the gate "Noire aux Champs." Peter states in his own account that he loaded the connoile with a quarrel, meaning in this case a heavy bolt, not an altercation. To the quarrel Peter affixed two pounds of lead. From the subsequent happenings there is reason to suppose that he did not omit powder. Peter "laid" the connoile so that it

pointed against a door and wall. The spectators heard a "cruel noise," but the antics of the connoile remain a mystery.

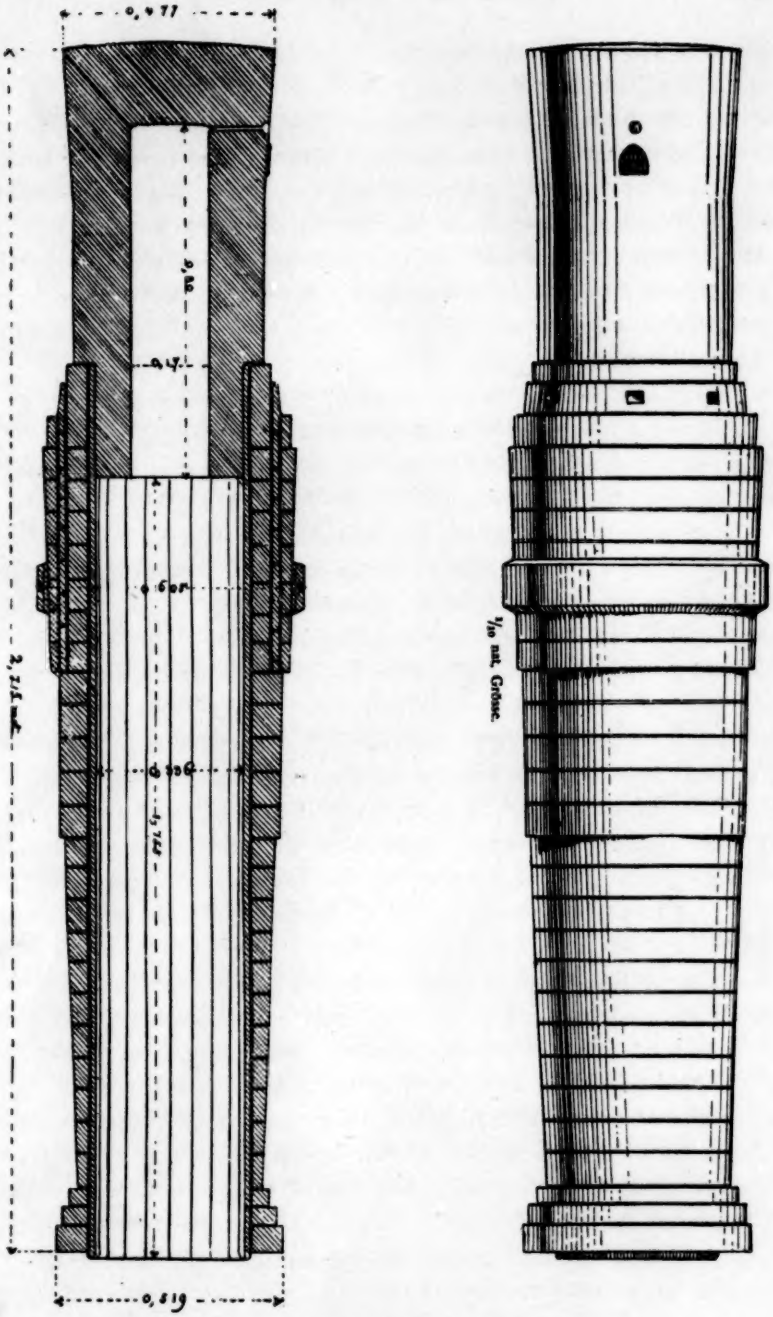
In quite another part of the city of Tournay, an industrious fuller was busily at work; when lo, along came the erratic quarrel, with its two pounds of lead,—and the guild of Fullers gave their deceased brother one of those picturesque funerals for which the good town is so celebrated. When Peter of Bruges heard of this mishap, he fled into sanctuary and gave himself up for lost. Then followed a solemn session of the consuls. Contributory negligence could not be charged against the Fuller, for if ever bolt came "from the blue" it was this one. After a long discussion the conclusion arrived at was: Peter of Bruges fired the connoile at the order of the consuls; he was not known to have harbored any ill feeling against the fuller;—they might have added that neither ill feeling nor skill in aiming would have enabled Peter to hit the far-off fuller. The consuls thereupon held Peter blameless, merely remarking that the event was a misfortune and a sad pity.

A curious point is brought out by the list, dated 1347, of artillery, in its broader sense, for the defense of the castle of Brioul in France. At the fag end of the list we are told that one man managed two cannons, and that the efficiency of their projectiles, and of stones thrown from the towers *by hand*, was considered about equal.

Before glancing at the great bombard of Caen, 1375, which marks a considerable step in advance, let me say that during the fifty years we have glanced at, there have been cannon of wrought iron, occasionally of brass. The largest of the former did not weigh over 120 lbs. Breech-loaders were common, and the projectiles were bolts, or balls of lead—iron balls are referred to, but never stone.

March 20, 1375, an order was received at Caen, in Normandy, from Jehan Le Mercier, one of the King of France's councillors, for the building of "a great cannon of iron." March 21 work began by erecting three forges in the market place, and surrounding them with a wooden paling to keep the curious at a proper distance.

March 22, the four smiths with their eight helpers began to draw wages. Fifteen men worked for six weeks, sometimes at night. April 3, Jehan Nicolle, a master smith, said to have been the best in Normandy, arrived from "Sap." 2,110 pounds of wrought iron,



Built-Up Wrought-Iron Bombard, 1420–1430.
Length 8 ft. 6 in. Caliber 13¼ in. Basel Arsenal.

and 200 pounds of steel were used. The inner tube was formed by longitudinal bars; encircling these were tight-fitting rings of iron, driven on, one touching the other, till they formed an unbroken surface. 400 pounds of the iron was Spanish, presumably a better quality. The "cuve" for which it was used, may have been that part of the breech which enclosed the powder chamber. 200 pounds of steel were needed; could the chamber have been of that metal? The chamber seems to have formed a permanent part of the bombard, as the vent is specially mentioned with its large projecting apron of iron.

After the metal part was finished ninety pounds of rope was wound about the gun, for what purpose we are not told. Over this was sewn a cover of hide to prevent the rope rotting or the metal rusting if exposed to rain.

The manner of attaching the monster to its heavy wooden bed and braces, is fully and confusedly described. General Favé thinks its carriage was a kind of cage, somewhat like that used by blacksmiths in France for shoeing unwilling animals. Four stone balls, size not given, were provided, at a cost of two sous six deniers each ($1.50? today). Two of these were used in the proof rounds.

After this date (1377) the size of cannon rapidly increased. Froissart mentions 140 cannon used at Odruik or Outherwyck, by the Duke of Burgundy, in 1377, which threw balls of 200 pounds. A work (name not given) professing to quote contemporaneous authority, mentions a cannon of the Duke of Burgundy, 1377, throwing a shot of 450 pounds, which would require a calibre of say, 21 inches.

1382, at the siege of Oudenarde by Philip van Arteveld, the Flemings made use of a "marvellously great bombard," so they said, at least. They added, that when this bombard was fired, by day it could easily be heard a distance of five leagues, and by night ten. It made such a terrible din (French "noise") that to those who listened, it seemed as if all the devils in hell were rushing on. The rather imaginative old chronicler says that this monster had "53 pouces de bec" (mouth). Englished, a trifle over 58 inches. Either we must credit him with having measured the circumference, —rather an unusual manner of classifying artillery, making the real

caliber only about 18 inches, or else,—but the alternative is too painful.

The accounts of the fighting about Chioggia, 1380, between the Genoese under Pietro Doria, and the Venetians under their beloved Vittore Pisano, are well authenticated, and give a vivid picture of the power of these old bombards. January 22, the great bombard, a two-hundred pounder, was fired by the Venetians at the campanile of Brondolo; it knocked out a large piece of wall, and some of the flying stones struck and killed Pietro Doria, the Genoese commander, together with his nephew. The next day the same bombard brought down a still greater piece of the same campanile, killing 22 men; so that as an implement of slaughter, the clumsy thing was a success and endeared itself proportionately to the Venetians.

Before leaving the fourteenth century a few short notes might be added.

The castle of Tannenberg, in Germany, was captured 1399. A huge bombard belonging to the city of Frankfort a/M., was loaned to the besiegers. Tremendous difficulties were met and overcome in getting the gun into position, very close to the castle. The first projectile stuck in the wall; the second passed through, and soon the defences were in ruins. These were never rebuilt. Excavations were made in 1849 and many stone balls were found. They varied in diameter from three inches to 31½ inches, the latter weighing 825 pounds, and unquestionably one of the shot for the "Frankfurter Buechse."[3]

Napoleon gives an inventory of the Artillery of Bologna, 1381/97, in which stone balls of 1,000 pounds for bombards and mortars, together with iron balls of 1, 2, 3 and 6 pounds are mentioned.

A word about *field-guns*. Froissart,[4] speaking of the capture of the castle of *la Roche sur Yon* (1369) by the Black Prince, mentions "several cannons and springalls with which the army was provided, and from long custom had always carried with them."

In the year 1382 the bumptious burghers of Bruges were engaged in one of their usual wars with their equally bumptious neighbors

[3] "Die Burg Tannenberg und ihre Ausgrabung," Hefner und Wolf, Frankfurt, a/M., 1850.

[4] Chap. 268, Vol. 1.

of Ghent, who took the field 5,000 strong with 200 "ribaudequins." The latter were heavy built push-carts—Napoleon calls them "wheel-barrows," bearing in front two or three, sometimes more, of the small cannon of the day, with an ugly fringe of bristling lances projecting beyond. These disagreeable field-pieces were trundled along in front of the line of battle. The effect of two lines of "ribaudequins" meeting and neutralizing each other must have given rise to some curious tactics in battle. In this case the 5,000 of Ghent formed themselves into a dense mass and with "ribaudequins" in front, drove off 40,000 men of Bruges.

At the battle of Roosebeke, November 27, 1382, where the Flemings were cut to pieces by the French and their leader Philip van Arteveld killed, Froissart states that the battle began by "a cannonade with bars of iron and quarrels headed with brass."

This battle did not end the war, and a curious picture of the ineffectiveness of the smaller cannon of the day is given by Lieut. Gen. Sir Henry Brackenbury, in his account of the siege of Ypres by the English and Flemings. The siege lasted from the eighth of June, 1383, to the eighth of August. During that time a steady cannonade was maintained, but apart from interfering with the sleep of the good burghers of Ypres, not a soul was one whit the worse. Two guns were advantageously posted in front of one of the gates, and kept up a steady fire, in all 450 shots. When the siege was raised those of Ypres were forced to admit that the gate in question was in need of immediate repairs. Much danger to the inhabitants was avoided by a thoughtful device; the besiegers considerately heralded by a trumpet blast each discharge; this enabled promenaders to step aside and avoid any possible annoyance from intruding cannon balls.

Another curious picture of by-gone days is given us in the "Issue Roll of the Exchequer for 1384," in which the amount of payments for the hire of cannon and cannoniers is given, making it plain that private individuals often owned one or more cannons which they hired out like cabs.

Viollet le Duc mentions this same custom on the continent; he says that during the middle ages the engines of war were made by non-military workmen, and the same rule prevailed after the intro-

duction of cannon. Not only did ordinary mechanics make the new artillery, they also served it; letting their cannon for hire as one lets carts and drivers; and it was not until the death of Charles VII, 1461, that they formed companies of bombardiers and culveriniers, heavy and light artillery, like the companies of cross-bowmen and archers, gave to them military organization, and placed them under the command of the grand master of artillery.

The fifteenth century was one of development, very important but less startling than its predecessor.

The most marked advance was in cast bronze and iron guns. Pretty much any date after 1400 may be taken as the beginning of that phase of the smelter's art. Erfurt claims 1377 as her beginning; it seems needlessly early, but no one can say her nay. There are two cast-iron guns in the Leipzig Museum, one between 1400 and 1420; another, less archaic, 1420 to 1430.

Francis Grose says:

> It seems extremely strange, that none of our workmen attempted to cast them, [cannon] till the reign of King Henry VIII. when in 1521, according to Stowe, or 1535 [Camden says], great brass ordnance, as canon (sic) and culverins, were first cast in England, by one John Owen, they formerly having been made in other countries; . . . 1543. . . [Stowe] . . . the King minding wars with France, made great preparations and provisions, as well of amunitions and artillery as also of brass ordnance; amongst which at that time, one Peter Bawd, a Frenchman born, a gun-founder, or maker of great ordnance, and one other alien, called Peter Van Collen, a gun-smith, both the King's freedmen, conferred together, devised and caused to be made, certain mortar pieces, being at the mouth, from eleven inches up to nineteen inches wide. . . . and after the King's return from Bullen [Boulogne], the said Peter Bawd by himself in the first year of Edward VI. [1547] . . . did also make certain ordnance of cast yron of diverse sorts and forms, as fawconets, falcons, minions, sakers, and other pieces. Chamber'd pieces for throwing stones, called cannon-perriers, port-pieces, stock-fowlers, sling-pieces, port-ingale-bases, and murtherers, were about this time much used in small forts and on shipboard.[5]

Of course all these guns were cast hollow; that is a core covered with clay, was suspended in the center of the mould while the metal was poured in. Despite all precautions it was very nearly impossible with the imperfect means then in use, to keep this core in place and true; cavities formed in the metal about it, and the scoria did not

[5] Francis Grose, "Military Antiquities," London, 1788, II., p. 383.

rise freely; certainly too much cannot be said in praise of the founders who could cast such a gun as the serpentine of Charles the Bold (say 1476), in the arsenal at Neuveville, near Bern; a cast-iron field gun some fifty-two inches long, and 2-inch bore.

Machinery for boring cannon is said to have been invented by Lew, in Switzerland. It was introduced into France 1740/44, by Jean Maritz, born in Bern (1711–1790), who, after accepting office under the French was naturalized. Maritz seems to have been the first who thought of placing the gun horizontal and making it, not the drill, revolve.

The Great Bombard, the characteristic gun of the latter half of the fourteenth century and the greater part of the fifteenth, was often, in its early days but a huge tube—"tuyeau de tonnerre." It is possible that after the frequent burstings, the occasional survivor noticed that these annoying accidents usually had their origin just in front of the chamber, about where the great stone ball was placed. The gun-maker would naturally strengthen this portion with much thicker bands, and doubtless he would soon deduce the fact that the strain decreased from the bursting point to the muzzle, then he would shape his gun to suit. The early gunners suffered terribly from the bursting of their guns. James II., King of Scotland, was killed at the siege of Roxborough Castle, 1460, in this manner; 1470, a bombard near Paris burst, killing 14 men and wounding as many more.

It was long before the early gunner discovered (the figures are for a 4.25 inch caliber) that the proportional pressure on the bore increased alarmingly with the weight of the ball; 3.6 per square inch for stone; 10 for iron; 10.9 for bronze; 14.5 for lead. For the same caliber; the cost for one round; 4-inch ball, charge 1–9 wt. of ball:—In money of to-day: with a stone ball, $1.25; iron, $4.75; lead, $6.25; bronze, $9.00.

Stone balls had two bad defects—they were apt to shatter to pieces when used for breaching purposes against heavy masonry; and their rough surface greatly damaged the interior of the bombard; it was sought to correct these defects; the first by bands of iron about the ball; the second by enclosing the ball in an envelop of soft lead.

The difficulties in the construction of these big bombards were greatly lessened by the system of forging the very heavy breech piece so that it could be screwed to the chase—and unscrewed, when desired; the square holes for the levers, that worked like capstan bars on board ship, are conspicuous in such guns, usually in the rear ring of the chase, and at the rear end of the breech. In some bombards there were three divisions, greatly adding to convenience in transportation.

Here are the dimensions of the largest bombard that has come down to us, the one on the Place du Marché at Ghent. The lady is called "Dulle Grete." which they tell me can be translated "Mad Meg." Her caliber is 26″; interior of chase 10′ 4″, or five calibers. Chamber caliber 10.23″, length 4′–6.16″; five to six calibers. The exterior length 16′ 6″. The gun is built up of 32 longitudinal iron bars, 2.17″ wide, 1.2″ thick; these are soldered together. Over them are 41 iron rings, welded together and diminishing in thickness from the junction with the breech to the muzzle, except the three which form the muzzle moulding or swell. In addition, there are 20 bands, called "rondelles," in two of which, the one at the extreme end of the breech and the one at the end of the chase, are holes for the levers used in unscrewing breech from the chase. Curiously enough, the breech is not exactly in line with the chase, inclining slightly to the left,—might possibly be a trifle trying for the right-hand side of the chase after a few shots. Meg's weight is 36,080 pounds, but painters of that day represent Flemish women of her class as distinctly heavy. The ball weighed 748 lbs., and the powder charge was 88 lbs., between ⅛ and ⅑ the weight of the projectile. The range was about 3,000 yards, at least Meg claimed that, though her effective range could not have been more than three hundred yards. But, it is only fair to remember that in Nelson's day six hundred yards was long fighting range.

The date of this huge, but rather useless engine of construction —destruction, I fear, would be gross flattery—is rather uncertain. The Flemings took it to the siege of Oudenarde, in 1452; a coat-of arms I found near the vent is that of the father of Charles the Bold, called "Philip the Good," because he was bad. He warred from 1422 until 1467.

At the siege of Caen, in 1450—this is all quoted from an account in very bad Latin—the town was ringed about with twenty-four bombards, horrible to behold, for they were of such immense size that a man could sit in any one of them without bending his head! Possibly this old chronicler's account was intended to fall into the the hands of the besieged—though of course, cannon of that size would make very comfortable quarters for at least 48 men.

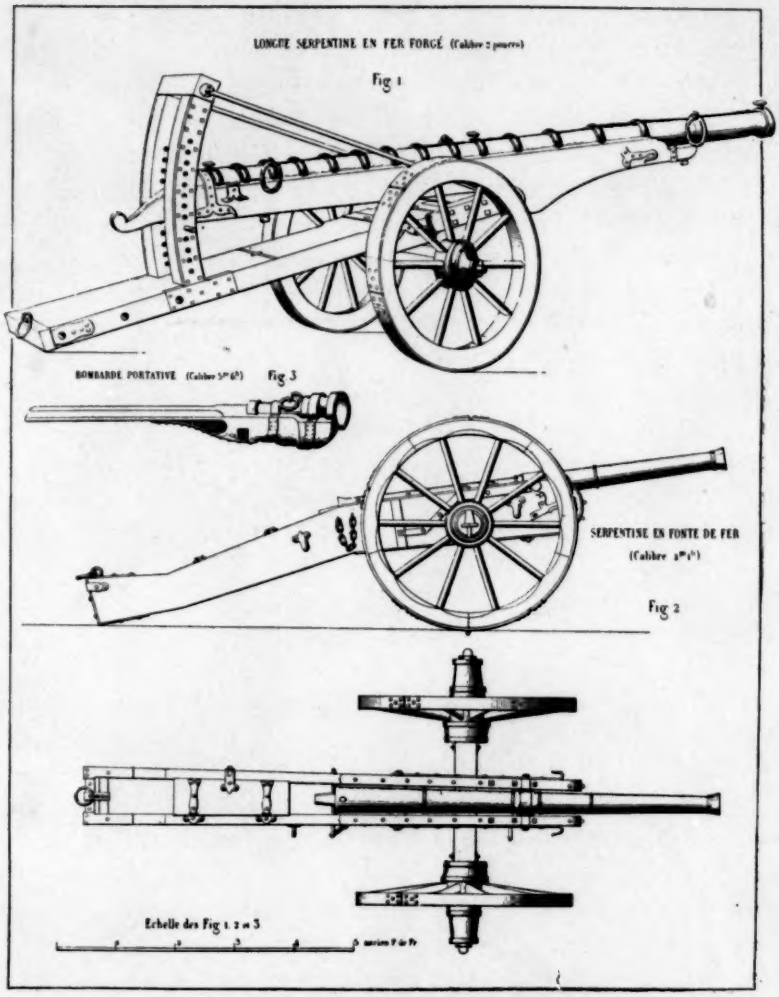
The preceding is but a dip into the doings of bombards; we have not touched on that most interesting one, "Mons Meg," at Edinborough Castle; nor the "Michelettes," at Mont St. Michel, which are of unique value; the "Faule Mette," was a cast bombard, a resident of Brunswick, but alas she has disappeared.

Wooden guns are of great interest, not only on account of the frequent tendency to burst which must have been theirs, but also on account of the very unreliable descriptions of them that we possess.

About 1450 trunnions appear (not the first, by any means) on some of the Burgundian guns, adding, of course, greatly to their efficiency, both in permitting more exact aiming and as a help to resist the recoil. Omitting all other technical details it may be of interest to take a look at the Burgundian, and afterwards at the French artillery.

The town of Neuss, near the Rhine, and not far from Duesseldorf, was unsuccessfully besieged by Charles the Bold, 1474/5. The Burgundian artillery, then the best in Europe, was well represented there, and more or less careful accounts of it have come down to us. The following list is taken from Napoleon's "Etudes"; unfortunately the calibers are omitted.

Nine large bombards. Eight bronze bombards, 8 to 11 ft. long; these had lions' heads at the muzzle. Ten courtaux, 4 feet long, on wheels. These were a little like the carronade of just before 1800, to forty or fifty years after; there are accounts of courtaux which carried 60-lb. balls and were used as siege pieces. 115 Serpentines, one of which was 13 feet long. Six serpentines of bronze, with dragon heads at the muzzle, one of these guns was 8 feet long. Sixty-six serpentines 6 to 9 feet long. Fifteen others of the same caliber weighing 4,000 pounds.



Field-Guns of Charles the Bold, c. 1476.

FIG. 1. More ancient build; wrought-iron bars, banded. FIG. 2. Cast-iron; modern looking gun and carriage. FIG. 3. "Portable bombard"; throws incendiary bomb or stone ball.

Like many other weapons, artillery is not of much service unless you know how to use it, and do not hesitate to use it. Charles the Bold was the last of the knights-errant, unless we include the chivalric Don Quixote. Cannon had changed all that and Charles was a failure, though a magnificent one.

March 2, 1476, was fought the battle of Granson; Charles of Burgundy had 20,000 men and his splendid train of field-artillery; both these he proceeded to post as badly as he conveniently could. The Swiss always attacked in solid squares, impervious to cavalry but just the food for cannon to devour. Nine thousand men, and absolute silence, save the word of command; instant death to whoever faltered. A few shots took effect on that solid human mass, as it moved slowly towards the guns, each ball mowing down ten or a dozen men; then a dip in the ground protected them, and the balls passed over their heads. Now was the time for Charles to have concentrated his artillery fire on the square and rent it to pieces, for his cavalry to drive off the field. Instead, time and again he launched his magnificent gendarmerie against that bristling wall of steel, those 16-foot spears held by sturdy mountaineers who knew not fear. Every attack failed, panic followed, and that splendid Burgundian artillery now adds interest to a score of Swiss museums.

Napoleon III. and General Favé consider the artillery of Charles VIII. the beginning of that arm of the French service. Of course guns of earlier days still lingered on, but the newer ones took on almost the form they were to retain for three hundred years.

Guns changed but little from 1500 until the astounding development of today. Drake fought the Spaniard with almost the same guns that Nelson used at Trafalgar.

A better organization, and an improvement in tactics was made by Charles of France, before his great Italian campaign of 1495. On the other hand he was opposed by very different foes from the heroes who defeated Charles of Burgundy at Granson, Morat and Nancy. One may safely say that France easily, almost pityingly scattered before her powerful guns the very worst troops the world contained at that time. Burgundy, on the contrary, had faced the bravest and best fighters history tells of. Swiss tactics, the old phalanx of Greece, steadily adhered to, soon became obsolete, and the system of rushing the guns with such unwieldy squares, received its death blow on the days of Marignano, 1515; when, cannon to the right of them, cannon to the left of them, cannon in front of them, volleyed and thundered. Two days of carnage failed to shake the Swiss; but when Francis I., massed his artillery, and the

Swiss attack was exposed to a cross fire that tore their squares to shreds,—just what Charles of Burgundy should have done at Granson, they sullenly fell back and the rule of the cannon began; alas, that its end is not yet in sight.

Ten years later this same Francis was routed and captured at Pavia; one reason for it was that he stupidly masked his own guns by advancing his troops in front of them; another, that many of the Swiss of Marignano were then fighting on his side; but those days of Marignano and the slaughter were not forgiven; so when the crisis came, the Swiss, despite the despair and entreaties of their officers, threw down their arms and pretended to be cowards,—for a Swiss it *could* only be *pretence*.

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MOREAU DE SAINT MERY AND HIS FRENCH FRIENDS IN THE AMERICAN PHILOSOPHICAL SOCIETY.

By JOSEPH G. ROSENGARTEN.

(*Read April 20, 1911.*)

Born at Fort Royal, Island of Martinique in 1750, dying at Paris in 1819, Moreau de St. Mery had a career characteristic of the stormy period through which he passed. Of a good family of Poitou, his father's early death left him with little means. At nineteen he came to Paris, became a King's gendarme, studied law, letters and mathematics. Returning to Martinique he became a lawyer at Cap Français, and in 1780 a member of the Upper Council of Saint Domingo. He classified the laws of the French Colonies of the Antilles; discovered and restored the tomb of Columbus, and sent many scientific papers and many curious archeological articles to the American Philosophical Society, and was elected a member in 1789.

Returning to Paris as a member of the Constituent Assembly from Martinique, he was warmly welcomed by the scientific world in recognition of his frequent contributions to scientific societies.

When the French Revolution broke out, he was elected President of the Electors of Paris, twice addressed Louis XVI. on their behalf, and was fond of boasting that for three days he had been King of Paris, and helped to secure for Lafayette the command of the National Guard.

Elected Deputy from Martinique in 1790, he brought many colonial matters before the Constituent Assembly, and in 1791 became a member of the Judicial Council.

Wounded in an attack by a maddened crowd, he took refuge in a country village in Normandy, escaped the guillotine and came to the United States. After a short stay in New York, he settled in Philadelphia in 1793, opened a book store at Front and Walnut Streets, and became active in the Philosophical Society, attended its meetings regularly, contributing papers, making gifts to its collections,

introducing many of his fellow exiles, some of them soon elected to the Society. Returning to France in 1799 and making use of his distant relationship to Josephine, wife of Bonaparte, he was employed by Bonaparte in the preparation of a Maritime Code. Appointed to the Council of State in 1800, he was sent in 1801 to Parma as Administrator of the Duchy of Parma, fulfilling his duties with moderation, but showing a lack of firmness and energy that cost him his position, and the enmity of Napoleon, who sent Junot to replace him, and to end a threatened revolution by fire and sword.

When he lost his place in the Council of State, he told Napoleon that his honesty need not be feared, for it was not contagious in that body. The Empress Josephine helped him, and afterwards he became historiographer of the Marine Department.

He sold to the French government, for a pension from Louis XVIII., his large collection of historical papers, documents, maps, etc., often mentioned by recent historians. One unkind critic, who worked at making a calendar of his papers, says he sold to the government not only the copies he had made, but many originals which he had taken from the files in his care. His printed works include one in six volumes, on "The Laws and Constitutions of the French Colonies in the West Indies from 1550 to 1785." Louis XVI. ordered a copy to be placed in each French colony in America.

His "History of Saint Domingo" was translated by William Cobbett, then living in Philadelphia, and his list of subscribers included many notable Americans then in office and a large number of French exiles in the United States.

He translated and published a pamphlet on "The Prisons of Philadelphia," by Rochefoucauld Liancourt, reprinted in Paris and in Holland, and in one of Rochefoucauld Liancourt's bulky six volumes of his "Travels in the United States." He had the honor of an eloge by Fournier Pescay printed in Paris in 1819 and the biographical dictionaries give the dates of his various publications, of the offices he held and make mention of his best service: the collection and preservation of an immense number of papers, maps, etc., relating to the French colonies in America, from their origin down to the French Revolution. Calendars of parts of them have been

printed by the Canadian Archive office and by the Wisconsin Historical Society and in France.

His shop at Front and Walnut was the rendezvous of all the notable French exiles then in Philadelphia, and he entertained them very modestly,—cooking his own simple meals in his rear office, and sharing his good wine with them. He figures in the "Memoirs" of Talleyrand and in the "Travels" of Rochefoucauld and in the books on the United States by Brissot and Volney and other French writers.

He translated and published two big quarto volumes on China by Van Braam, who had resided in that country as a member of a Dutch embassy. The book was dedicated to Washington. Van Braam became a merchant in Charleston in 1783 and was naturalized in 1784, then made his voyage to China, returned to Philadelphia in 1796, bringing with him several Chinese servants, and a large collection of paintings, drawings, maps and curios, which he exhibited in Philadelphia for several months, then kept in his house near Bristol, "China Hall" on the Delaware. In the appendix to the second volume of his book, there is a detailed account of his collection, filling many pages. He too was elected a member of the Philosophical Society in 1797.

Moreau de St. Mery printed a catalogue of the contents of his book store, of 72 pages, including many books in English, French, Italian, Spanish, Latin, German, Dutch, maps, music, and advertised "a general business of stationers, booksellers and dealers in engravings, a printing office and book bindery, to fill orders for books from Europe, deal in every kind of business on commission, and will not spare any care in studying to accomplish their enterprise intended to propagate and diffuse knowledge," and at the end of the catalogue of books, etc., offered for sale, "particular goods out of the booksellers' station, everything belonging to the Fleecy hosiery manufacture of New York, as foot and ankle socks, goutty mittens and stockings, shirts with and without sleeves, drawers, muffs, etc., elastic garters and gallices of different sizes." Perhaps his field was too large, and the public not appreciative, for he failed for $5,000, and Philadelphia lost the advantage of such a bookseller, printer and publisher, as well as philosopher, author and translator.

No doubt the same industry and energy led him to make the large collection known by his name, now in Paris, of original documents, copies, maps, etc., filling 287 volumes, bought by the French government, and now in its great archives for the use of students of colonial French history.

That St. Mery was well thought of in Philadelphia, during his residence here is attested by the long list of subscribers to his book on Saint Domingo, including Vice-President John Adams, Adet, the French minister, Benjamin Franklin Bache, William Bingham, Thomas Bradford, Samuel Breck, Rev. Dr. Collin, Alexander James Dallas, P. S. Duponceau, Dupont, of Wilmington, Rufus King, Dr. Logan, Noailles, Timothy Pickering, Rochambeau, Talleyrand, John Vaughan, Volney, and many notable French exiles both in Philadelphia and elsewhere in the United States. Many of them were elected members of the American Philosophical Society, and its minutes show that its meetings were frequently attended by Talleyrand, Rochefoucauld, Volney, Van Braam, and its library has many books, the gifts of St. Mery and his fellow exiles.

In a recent biography of Talleyrand we are told that when he landed here in 1794, it was the finest city in the United States, full of life, everywhere new buildings and work on them going on, the streets full of elegant equipages, crowded with men of business, workmen and sailors.

Chateaubriand speaks of the beauty of the Quakeresses. Every stranger from Europe was welcomed by the wealthy merchants,—life was very expensive, board $8 to $12 a week, without fire, light or wine; a negro servant cost $10 to $12 a month even with food and washing. Emigrés of all political creeds found a Noah's ark of refuge in Philadelphia. Talleyrand's arrival was quite an event; he found old friends, old soldiers of Lafayette, fellow members of the constituent assembly, among them Blacon, who had been deputy from Dauphine and one of the intermediaries between Mirabeau and the King. Hamilton gave him a warm welcome, but Fauchet, the French Minister, prevented Washington from receiving him, and Washington wrote to Lord Lansdowne, explaining why his letter of introduction did not enable him to meet Talleyrand. However, he

did not busy himself with politics, but at once began speculating in land, then the great money-making business. The Scioto Company was then all the vogue in Paris. The Holland Land Company was buying right and left. LaForest, the French Consul General, had bought an estate in Virginia in 1792. Noailles and Omer Talon in association with Robert Morris had bought large tracts of land on the Susquehanna for a colony of French royalist exiles, offering land, which had cost them 15 sous an acre for 6 francs, as a refuge from France.

Talleyrand urged Mme. deStael and his friends in Europe to send money for investment, and he proposed buying land in Maine from General Knox. He told Moreau de St. Mery that he had a plan for settling in Louisiana, and was a frequent visitor at St. Mery's book store, meeting there his old friends and fellow exiles,—Fayettists, Girondists, Constituents, Jacobins, Royalists, one of them, Count de Moré, says "wandering like ghosts, full of regrets, lost hopes and disappointment over their shattered political careers."

Moreau de St. Mery often spoke of the three days in 1789 when as president of the Electoral College he was King of Paris. But while the others were bewailing their hard fate, Moreau was busy with his shop and his books, and Talleyrand wrote to Paris of schemes for revictualling Paris, starved by the Reign of Terror, crying for bread, by ships loaded with rice, grain and fish, named the best merchants to deal with, and on the strength of his services, secured the long-sought permission to return to France, and there began that career of success which carried him safely through the Republic, the Directory, the Empire, the Bourbon restoration and into the reign of Louis Philippe.

Other Frenchmen had planned a great French colony,—twenty-four men, mostly young noblemen, had joined in forwarding Joel Barlow's scheme of a great settlement on the Ohio,—the Scioto Company was organized, to buy 24,000 acres,—d'Epresmenil, their leader, lost his life on the guillotine; Marnesia, after a tour through America, returned to France, and with Lally Tollendal, Mounier and Malouet, lost touch with their colony in the midst of the great events in their own country.

One of Moreau de St. Mery's friends and visitors, Rochefoucauld Liancourt, wrote an account of the prisons of Philadelphia, which was printed by Moreau in French and in English in Philadelphia, (he was the translator), and later it was published in Paris, and in Dutch in Holland, and later still was made part of one of his six volumes describing his travels in the United States.

Rochefoucauld spent four years in the United States and describes in great detail his experiences in the northwest and north, in Canada, in Maine, in the south, and in New York, New Jersey and Pennsylvania.

Talleyrand from Philadelphia wrote to Mme. de Genlis, "Rochefoucauld is here, making notes, asking information, writing, and more a questioner than Sterne's curious traveller; he wants to see and know everything," in his eager search for the truth. He met Knox, Sullivan, Jefferson, John Adams, Priestley, Livingston and Kosciusko. He appealed to Washington to intercede for the release of Lafayette from Olmutz. His inquiries included politics, constitutions, judicial organizations, army, agriculture, industries, statistics, charities, education. In Georgia he studied cotton and indigo plantations; he condemned slavery and argued for the education of the negroes to prepare them for freedom; in Niagara and the great forests he foresaw the sources of future industries. He established in France on his return societies like the Pennsylvania Prison Society, and took home much that he had learned in the United States, which he introduced in France, useful reforms that made him a real philanthropist.

Another French settler in or near Philadelphia, Pierre Legaux, was elected a member of the American Philosophical Society in 1787. A Counsellor of Parliament, a member of the Academy of Arts and Sciences and of several foreign academies, employed in the French West Indies, he came to Philadelphia about 1786 and made his mark as a representative of French culture and scientific ability and by his charm of manner. He bought land on the Schuylkill near Conshohocken and planted vineyards. Washington and Mifflin and other notable men visited them and approved his enterprise. Jefferson, Genet, Brissot, Audubon, Wistar, were among those whose visits

and encouragement he recorded in his diary. He tried to get Jefferson to recommend to Congress protection for his infant industry. In 1791 he offered his country house to Washington as a home during the session of Congress and "hoped the country which owes its liberty to your wisdom and military talent will owe her wine to your generosity."

In 1793 the Legislature of Pennsylvania chartered a company to promote the cultivation of vines, with a capital of $20,000 in $2 shares; in 1800 the stock was fixed by a law passed by the Legislature at $1 a share down, and the balance of the $20 in easy instalments. Later he advertised that apprentices, black or white, would be received, with terms of payment, and the promise of a gift of vines that they could take home and start the industry wherever they lived. In 1802 the company received its charter and organized. Among the stockholders were Thomas McKean, Robert Morris, Genet, Duponceau, Stephen Girard, Alexander Hamilton, Aaron Burr, Jared Ingersoll, Muhlenberg, Bartram and other notable people of the 385 subscribers to the stock of the Pennsylvania Vine Co. Legaux was elected superintendent at a salary of $600 a year with residence and living at the farm. Expenses soon outran receipts, the managers quarreled with Legaux, litigation brought ruin, and he, harassed, worried, disappointed, became a mere servant where he had once been a genial host, finally succumbed and broken in spirit died in 1827, and was buried at Barren Hill. Thus sadly closed another one of the frequent failures of French enterprises in the United States.[1]

Moreau de St. Mery kept a journal, cited by Pichot in his "Souvenirs intimes de Talleyrand," in which he speaks of Talleyrand's frequent visits to his book store, meeting there Noailles, Rochefoucauld, Omer Talon, Volney and others less famous.

While the host dined meagerly on rice and milk cooked in his store, Talleyrand enjoyed drinking his own old Madeira, and was the life of the party. When Blacon called him monseigneur all the company burst into a hearty laugh. Talleyrand urged Napoleon to erect a statue of Washington in Paris and to give France the

[1] *Philadelphia Press*, September 9, 1899, article by Samuel Gordon Smythe.

same perfect religious freedom that he saw practiced in the United States and he also advised the sale of Louisiana to the United States as a method of strengthening the ties between the two countries.

One would like to see the journal kept by Moreau de St. Mery during his residence in Philadelphia. Did he in his palmy days as a member of the Council of State under the Empire or in the time of his modest clerkship in the Marine Department, meet his old visitors at his book store in Philadelphia, Louis Philippe, Talleyrand and Rochefoucauld and Volney and the other exiles now restored to their old prosperity, and did they recall the meetings of the American Philosophical Society and their attendance and share in them? His large collection of historical papers, now rescued from oblivion by calendars by and for the American students of history, perpetuates his name and memory and services, more than do the volumes he wrote and printed and published at his book store at Front and Walnut Streets.

The latest historian of the French Revolution, Aulard, frequently mentions Moreau de St. Mery and his share in it, and refers to the collection of historical documents. His name does not figure in Dr. Mitchell's capital novel, "The Red City," with its picturesque account of the French exiles living here in the closing years of the eighteenth century, nor in Kipling's picturesque story of Philadelphia at that time. All the more reason therefore for an attempt to recall the memory of the French exiles who were members of the American Philosophical Society and especially of that one who figures most often and most usefully in its records of that time, Moreau de St. Mery.

Of the other French exiles during their residence in Philadelphia, there is occasional mention, as for instance in Talleyrand's "Memoirs." His two papers on the United States and the relations between France and this country, read before the French Institute, were no doubt largely inspired by what he heard at the meetings of the American Philosophical Society, and his share in the sale of Louisiana to the United States helped to secure that vast territory for the future growth of the young republic and its ultimate great development.

In the report of M. E. Richard on the Moreau de St. Mery collection, printed in the supplement to Dr. Brymner's Report on Canadian Archives for 1899 (Ottawa, 1901), he says it was stored in the archives of the Marine at Versailles up to 1887, then removed to the Ministere des Colonies, and stored in the attic of the Louvre. They were then fearful of the great risk of fire, but were considering the removal to other quarters.

In the reports of 1883–85 and 1887, mention is made of 287 volumes in the collection of Moreau de St. Mery, some forty of which relate to Canada, others to Louisiana and the French islands of America. These belonged formerly to the Colonial Archives of the Marine; of the collection headed Moreau de St. Mery seventeen volumes contain description, etc., of the colonies, including a series of memorials on Canada, 3 volumes are on the religious missions of Canada, 12 volumes on Newfoundland, 12 volumes containing royal instructions to governors, and decrees relating to Canada, 119 registers on Canada, Acadia, etc., 6 volumes on civil status of Canada, 34 volumes on Louisbourg; an analysis was made of 17 volumes of the Moreau de St. Mery collection for the Canadian Archives.

It is open to the objection that "there is no strict order followed in the compilation; it contains but a limited number of documents, or even extracts from documents. It is difficult to understand the dominant idea of this collection."

This collection is, nevertheless, most valuable, for it contains a considerable number of important papers, both transcripts and originals, not to be found elsewhere.

On p. 5 of Richard's Report, in a footnote, it is said Moreau de St. Mery, born in Martinique in 1750, studied law in Paris and practiced in St. Domingo, where he became a member of the Superior Council of the Island. Entrusted by Louis XVI. with the compiling of a colonial code, he published in Paris "Les Lois et Constitutions des Colonies Françaises de l'Amerique sous le Vent." Representing Martinique in the Constituent Assembly, he drafted the report of the Committee on the Colonies. Forced by political events to leave France, he fled to Philadelphia, where he remained from 1793 to

1798, employing himself as a bookseller and publisher. He there published his "Description de la partie Espagnole de St. Domingue," which he signed "Moreau de St. Mery, member of the Philosophical Society of Phila." He also translated or edited foreign works, and among them VanBraam's "Voyage to China." Having returned to France on the 18 Brumaire, he was, through his relationship with Josephine de Beauharnais, appointed in 1800 to the position of Historiographer of the Marine. Napoleon appointed him to the Council of State, in view of his knowledge of colonial affairs. In 1802 he was administrator of Parma and Guastalla, but lost favor and was removed in 1806. He died poor and in receipt of a pension from Louis XVIII.

While entrusted with a mission in St. Domingo, as publisher in Philadelphia and historiographer in Paris, we find him everywhere an observer and a worker, taking notes on everything. His collection of manuscripts comprises 287 large volumes, and was purchased by the state after his death, that is to say the government had to pay not only for the transcripts he had caused to be made, but even for the originals he had appropriated.

Persons who take a special interest in the social and religious condition of the country, the disputes and conflicts between the authorities will find in the Moreau de St. Mery collection far more than they could find in any other series.

That Moreau de St. Mery did a good work in preserving and making his collection is shown by the statement (in Richard's Report, p. 8, etc.), that the Archives of the Ministry of Marine were so utterly neglected that the precious papers were used during five weeks of the winter in 1793, as fuel to feed the stoves of the post of the Garde Nationale in the building where the archives were kept, and in 1830 an employee gave up the archives to pillage and sold, by weight for his own profit, whole piles of documents, bought by autograph collectors.

Thanks to the suggestion of Prof. Cleveland Abbe, I found in the *Monthly Weather Review* for February, 1906 (Washington, Weather Bureau, 1907), at pp. 64, etc., in a notice by C. Fitzhugh Talman of the U. S. Weather Bureau, the following: "Foremost among the early

writers upon the island of Santo Domingo, was Mederic Louis Elie Moreau de St. Mery, who produced three voluminous works upon the French possessions in the West Indies. Born at Fort Royal, Martinique, in 1750, he passed his early manhood in Haiti, and settled at the then capitol of the colony, Cap Français (now Cap Haitien). He held an important office in the administration of the colony, and also, under a commission from Louis 16th, travelled extensively through the French West Indies, collecting material for a work published in 1785, under the title "Lois et Constitutions des Colonies Françaises de l'Amerique sous le vent, de 1550 à 1785." Returning to France he took an active part in the French Revolution, until obliged to flee from his political enemies to the United States. It was during a period of exile in the latter country that he published two works descriptive of the island of Santo Domingo, one devoted to the Spanish part of the island, the other to the French part. Published by himself in Philadelphia in 1797, it was republished in Paris in 1875 by Morgand in 2 vols. 8vo. It is to this day regarded by the Haitians as the highest authority upon the physical geography of their country and is quoted at length in the latest Haitian gazeteer (Ronzier Dic. geog. et admin. d. Haiti, Paris, 1899). Mr. Talman reproduces St. Mery's chart of the Island, and a full abstract of his description of its meteorology.

Moreau de St. Mery was active in the Philadelphia Society of Cap Français, and in the Library of the American Philosophical Society there is the 1st vol. of its Proceedings,—no 2nd or later volume is preserved,—it shows that Moreau de St. Mery was the leading spirit in its activities. That his meteorological observations of San Domingo during his residence there in the eighteenth century, should be found of value today, is but another proof of his useful activity. His chief monument however is his collection, bearing his name, of original documents on the French in America, and by it he is now made known to students in the pages of Aulard, Brymner, Thwaites and other historians.

THE NEW HISTORY.

By JAMES HARVEY ROBINSON.

(*Read April 22, 1911.*)

I propose to discuss in this paper the value of historical study. The question has long haunted me and certainly merits a more careful consideration than it has, so far as I can discover, hitherto received. It will be impossible to do more here than to analyze the problem and briefly state the general conclusions which that analysis suggests.

The older traditional type of historical writing was narrative in character. Its chief aim was to tell a tale or story by setting forth a succession of events and introducing the prominent actors who participated in them. It was a branch of polite literature, competing with the drama and fiction, from which, indeed, it differed often only in the limitations which the writer was supposed to place upon his fancy. As Professor McMaster has recently said: "It was by no mere accident that Motley began his literary career with a novel called "Merry-Mount," and Parkman his with "Vassall Morton." These bespoke their type of mind. The things that would interest them in history would be, not the great masses of toiling men, not the silent revolutions by which nations pass from barbarism to civilization, from ignorance to knowledge, from poverty to wealth, from feebleness to power, but the striking figures of history, great kings and queens, the leaders of armies, men renowned for statescraft, and the dramatic incidents in the life of nations. Each must have his hero and his villain, his plots, conspiracies and bloody wars. Just as Froude had his Henry VIII.; just as Macaulay had his William III., Carlyle his Robespierre and Cromwell, and Thiers his Napoleon, so Motley had his William of Orange and Philip of Spain; Prescott his Cortez, Pizarro, Ferdinand and Isabella; and Parkman his Pontiac, Frontenac and La Salle. History as viewed by writers of this

PROC. AMER. PHIL. SOC., L, 199 L, PRINTED JUNE 27, 1911.

school is a series of dramas in each of which a few great men perform the leading parts and use the rest of mankind as their instruments."[1] The commonly accepted definition of history was long, "a record of past events" and these, naturally, the most startling and romantic and the best adapted for effective literary presentation. Doubtless there was some serious effort to describe conditions and institutions, since they formed the necessary setting for the events and anecdotes; sometimes they would even be assigned a place on their own intrinsic merits; but what may be called the epic ideal of history prevailed until perhaps fifty or sixty years ago when, owing to the influence of the modern scientific spirit, a very fundamental revolution became apparent.

Now let us review, by way of preliminary, what were deemed the advantages of the study of history of this older type. Lord Bolingbroke in his "Letters on the Study of History," written about 1737, says: "An application to any study that tends neither to make us better men and better citizens, is at best but a specious and ingenious sort of idleness; . . . and the knowledge we acquire by it is a creditable kind of ignorance, nothing more. This creditable kind of ignorance is, in my opinion, the whole benefit which the generality of men, even the most learned, reap from the study of history: and yet the study of history seems to me of all others the most proper to train us up to private and public virtue." History, he quite properly says, is read by most people as a form of amusement, as they might play at cards. Some devote themselves to history in order to adorn their conversation with historical allusions,—and the argument is still current that one should know enough of the past to understand literary references to noteworthy events and persons. The less imaginative scholar, Bolingbroke complains, satisfies himself with making fair copies of foul manuscripts and explaining hard words for the benefit of others, or with constructing more or less fantastic chronologies based upon very insecure data. Over against these Bolingbroke places those who have perceived that history is after all only "philosophy teaching by example." For "the exam-

[1] "The Present State of Historical Writing in America," reprinted from the *Proceedings of the American Antiquarian Society* for October, 1910; Worcester, 1910, p. 18.

ples which we find in history, improved by the lively descriptions and the just explanations or censures of historians," will, he believes, have a much better and more permanent effect than declamation, or the "dry ethics of mere philosophy." Moreover, to summarize his argument, we can by the study of history enjoy in a short time a wide range of experience at the expense of other men and without risk to ourselves. History enables us "to live with the men who lived before us, and we inhabit countries that we never saw. Place is enlarged, and time prolonged in this manner: so that the man who applies himself early to the study of history may acquire in a few years, and before he sets foot in the world, not only a more extended knowledge of mankind but the experience of more centuries than any of the patriarchs saw." Our own personal experience is doubly defective; we are born too late to see the beginning, and we die too soon to see the end of many things. History supplies in a large measure these defects.

There is of course little originality in Bolingbroke's plea for history's usefulness in making wiser and better men and citizens. Polybios had seen in history a guide for statesmen and military commanders; and the hope that the conspicuous moral victories and defeats of the past would serve to arouse virtue and discourage vice has been urged by innumerable chroniclers as the main justification of their enterprises. To-day, however, one would rarely find a historical student who would venture to recommend statesmen, warriors and moralists to place any confidence whatsoever in historical analogies and warnings, for the supposed analogies usually prove illusive on inspection and the warnings, impertinent. Whether or no Napoleon was ever able to make any practical use in his own campaigns of the accounts he had read of those of Alexander and Cæsar, it is quite certain that Admiral Togo would have derived no useful hints from Nelson's tactics at Alexandria or Trafalgar. Our situation is so novel that it would seem as if political and military precedents of even a century ago could have no possible value. As for our present "anxious morality," as Maeterlinck calls it, it seems equally clear that the sinful extravagances of Sardanapalus and Nero, and the conspicuous public virtue of Aristides and the Horatii, are alike impotent to promote it.

In addition to the supposed uses of history mentioned by Bolingbroke there was the possibility of tracing the ways of God to man. Augustine had furnished the first great example of this type of narrative in his "City of God" and thereafter history had very commonly been summoned to the support of Christian theology. Bossuet, writing for the Dauphin in the latter part of the seventeenth century, says: "Mais souvenez-vous, Monseigneur, que ce long enchaînement des causes particulières qui font et défont les empires dépend des ordres secrets de la Providence. Dieu tient du plus haut des cieux les rênes de tous les royaumes; il a tous les coeurs en sa main; tantôt il retient les passions, tantôt il leur lâche la bride, et par là il remue tout le genre humain. Veut-il faire des conquérants; il fait marcher l'épouvante devant eux, et il inspire à eux et à leurs soldats une hardiesse invincible. Veut-il faire des législateurs; il leur envoie son esprit de sagesse et de prévoyance; il leur fait prévenir les maux qui menacent les états, et poser les fondements de la tranquillité publique. Il connoît la sagesse humaine, toujours courte par quelque endroit; il l'éclaire, il étend ses vues, et puis l'abandonne à ses ignorances; il l'aveugle, il la précipite, il la confond par elle-même; elle s'enveloppe, elle s'embarrasse dans ses propres subtilités, et ses précautions lui sont un piége. Dieu exerce par ce moyen ses redoutables jugements, selon les régles de sa justice toujours infallible."[2] It was assumed by such writers as Bossuet that in spite of the confessedly secret and mysterious character of God's dispensations it was nevertheless quite possible for the skilled theologian to trace them with edifying confidence and interpret them as divine sanctions and disapprovals, blessings and punishments, trials and encouragements. For various reasons, which it is unnecessary to review here, this particular method of dealing with the past and deriving useful lessons from it finds few educated defenders at the present day.

In the eighteenth century a considerable number of "philosophies of history" appeared and enjoyed great popularity. They were the outcome of a desire to seize and explain the general trend of man's past. Of course this had been the purpose of Augustine and Bossuet

[2] "Discours sur l'histoire universelle," concluding chapter.

but Voltaire devoted his "Philosophie de l'histoire" (1765) mainly to discrediting religion as commonly accepted; and instead of offering any particular theory of the past he satisfied himself with picking out what he calls *les vérités utiles.* He addresses Madame du Châtelet in the opening of his "Essai sur les Moeurs et l'esprit des nations" as follows: Vous ne cherchez dans cette immensité que ce qui mérite d'être connu de vous; l'esprit, les moeurs, les usages des nations principales, appuyés des faits qu'il n'est pas permis d'ignorer. Le but de ce travail n'est pas de savoir en quelle année un prince indigne d'être connu succéda a un prince barbare chez une nation grossière. Si l'on pouvait avoir le malheur de mettre dans sa tête la suite chronologique de toutes les dynasties, on ne saurait que des mots. Autant il faut connaître les grandes actions des souverains qui ont rendu leurs peuples meilleurs et plus heureux, autant on peut ignorer le vulgaire des rois, qui ne pourrait que charger la mémoire. . . . Dans tous ces recueils immenses qu'on ne peut embrasser, il faut se borner et choisir. C'est un vaste magazin où vous prendrez ce qui est à votre usage.[3] Voltaire's reactions on the past were naturally just what might have been expected from his attitude toward his own times. He drew from "le vaste magazin" those things that he needed for his great campaign, and in this he did well, however uncritical his criticism may at times seem to a modern historical student.

Herder in his little work, "Auch eine Philosophie der Geschichte zur Bildung der Menschheit. Beitrag zur vielen Beitragen des Jahrhunderts" (1774), condemns the general lightheartedness and superficiality of Voltaire and other contemporary writers who were, he thought, vainly attempting to squeeze the story of the universe and man into their puny philosophic categories. Ten years later he wrote his larger work, "Ideen zur Geschichte der Menschheit," in which he strove to give some ideal unity and order to the vast historic process, beginning with a consideration of the place of the earth among the other heavenly bodies, and of man's relations to the vegetable and animal kingdoms. "If," he says, "there be a god in nature, there is in history too; for man is himself a part of creation, and in his wildest extravagances and passions must obey laws not

[3] "Avant propos."

less excellent and beautiful than those by which all the celestial bodies move. Now as I am persuaded that man is capable of knowing, and destined to attain the knowledge of, everything that he ought to know, I step freely and confidently from the tumultuous scenes through which we have been wandering to inspect the beautiful and sublime laws of nature by which they have been governed." Humanity is the end of human nature, he held, and the human race is destined to proceed through various degrees of civilization in various mutations; but the permanency of its welfare is founded solely and essentially on reason and justice. But it is a natural law that "if a being or system of beings be forced out of the permanent position of its truth, goodness and beauty it will again approach it by its internal powers, either in vibrations or in an asymptote, as out of this state it finds no stability."[4] Herder formulates from time to time a considerable number of other "laws" which he believes emerge from the confusion of the past. Whatever we may think of these "laws" he constantly astonishes the modern reader not only by his penetrating criticism of the prevailing philosophy of his time but by flashes of deep historical insight. He is clearly enough the forerunner of the "Romantic" tendency that culminated in Hegel's celebrated "Philosophy of History" in which the successive migrations and national incarnations of the *Weltgeist* are traced to its final and highest medium of expression, the German people.

These genial speculations of the philosophers of history rested usually upon no very careful study of historical sources and their conclusions seem to us now very hazardous, even if we grant the correctness of the data upon which they relied. It was inevitable that the historical students who, about the middle of the nineteenth century, commenced to feel the influence of the general scientific spirit of the period, should begin to look very sourly upon the earlier attempts to bring order and beauty out of a mass of historic assertions which were so commonly either erroneous or unproved, and to establish laws for events which one could not be sure had ever happened. The reaction against the dreams of the philosophers of history was, and is still, very clear. What may be called, for convenience, the "scientific" modern school of historians believe that history,

[4] Opening sections of Book XV.

like all other forms of scientific research, should be pursued first and foremost for its own sake. The facts must be verified and classified by the expert, without regard to any possible bearing which his discoveries may have upon our attitude toward life and the proper way of conducting it. Attempts to draw lessons from the past have, it is plausibly maintained, produced so reckless a disregard of scientific accuracy and criticism, that the prudent historian will confine himself to determining "how it really was"—an absorbing and delicate task which will tax his best powers.

Along with more exacting criticism and the repudiation of supernatural considerations and explanations came a revulsion against the older epic or dramatic interest in the past. The essential interest and importance of the normal and homely elements in human life became apparent. The scientific historian no longer dwells by preference on the heroic, spectacular, and romantic episodes, but strives to reconstruct past *conditions*. This last point is of such importance that we must stop over it a moment. History is not infrequently still defined as a record of past *events* and the public still expect from the historian a *story* of the past. But the conscientious historian has come to realize that he cannot aspire to be a good story teller for the simple reason that if he tells no more than he has good reasons for believing to be true his story is usually very fragmentary and uncertain. Fiction and drama are perfectly free to conceive and adjust detail so as to meet the demands of art, but the historian should always be conscious of the rigid limitations placed upon him. If he confines himself to an honest and critical statement of a series of events as described in his sources it is usually too deficient in vivid authentic detail to make a presentable story. The historian is coming to see that his task is essentially different from that of the man of letters. His place is among the scientists. He is at liberty to use only his scientific imagination, which is surely different from a literary imagination. It is his business to make those contributions to our general understanding of mankind in the past which his training in the investigation of the records of past human events especially fit him to make. He esteems the events he finds recorded not for their dramatic interest but for the light that they cast on the normal and prevalent conditions which

gave rise to them. It makes no difference how dry a chronicle may be if the occurrences that it reports can be brought into some assignable relation with the more or less permanent habits and environment of a particular people or person. If it be the chief function of history to show how things come about—and something will be said of this matter later—then events become for the historian first and foremost evidence of general conditions and changes affecting considerable numbers of people. In this respect history is only following the example set by the older natural sciences—zoology dwells on general principles not on exceptional and startling creatures or on the lessons which their habits suggest for man. Mathematics no longer lingers over the mystic qualities of numbers, nor does the astronomer seek to read our personal fate in the positions of the planets. Scientific truth has shown itself able to compete with fiction, and there appears to be endless fascination for the mind in the contemplation of what former ages would have regarded as the most vulgar and tiresome commonplace.

In addition to the characteristics of modern history just enumerated two great historical discoveries of the latter half of the nineteenth century have served still further to revolutionize our attitude towards the past of mankind. Curiously enough neither of these discoveries are due to historians. I refer to the well substantiated fact that man is sprung from the lower animals, and secondly, that he has in all probability been sojourning on the globe for several hundreds of thousands of years. These discoveries have gravely influenced all speculations in regard to the earlier history of our race and have placed the so-called "historical period" in a new setting. The historian no longer believes that he knows anything about man from the very first but realizes that what is commonly called history comprises only a very recent and very brief period in man's development. All history is modern history from the standpoint of prehistoric anthropology. Lastly, a group of anthropological, psychological and social sciences have made their appearance during the past fifty years which are furnishing the historian with many new notions about man and are disabusing his mind of many old misapprehensions in regard to races, religion, social organization, and the psychology of progress. The older historians used such words as

race, human nature, culture, religion, church, people, Renaissance, Reformation, Revolution, almost as if they were the names of animistic forces. These terms must be analyzed and reinterpreted in the light of the newer sciences of man.

The kind of history, accordingly, the practical value of which we shall attempt roughly to estimate, and which for convenience sake we may call the "new" history, is scientific in its methods, exacting in regard to the inferences it makes from its material; it rejects supernatural explanations and an anthropocentric conception of the universe; it studies by preference the normal and long enduring rather than the transient and exceptional; it accepts the descent of man from the lower animals, many of whose psychological traits he shares; it recognizes that man has lived on the earth for not merely five thousand but perhaps for five hundred thousand years; it avails itself, when fully abreast of the time, of all the suggestions and criticisms that are constantly being contributed by the newly developed sciences of anthropology, comparative, social and functional psychology, comparative religion, etc.[5] So much for the attitude of mind of the modern historian who realizes the changes which have overtaken his subject during the past fifty or sixty years.

But if "history" be re-defined as no longer a record of past events but the attempt to describe with all possible scientific precision what we know of the nature and conditions of human institutions, conduct and thought in the past, does not the term become hopelessly vague—as vague at least as the term natural science? Does not the historian sacrifice his only obvious clue to the past when he gives up tracing a succession of conspicuous events, for only these lend themselves to an obvious and orderly selection and arrangement? Every human interest and achievement has its history, every accomplished, and every vain dream. It would seem as if every attempt to deal with the past must necessarily imply an arbitrary selection dictated by the investigator's particular humor and tastes. This situation is still disguised by the continued popularity of a *standard* variety of history, mainly political, dynastic and military, transmitted to us from the past and taught in our

[5] See "The Relation of History to the Newer Sciences of Man" in *The Journal for Philosophy, Psychology and Scientific Methods,* Vol. VIII., No. 6, March, 1911, where I have elaborated this point.

schools and colleges and presented to the adult public in many well known older and newer treatises.

In order to appreciate the arbitrary nature of the selection of historic facts offered in these standard text books and treatises, let us suppose that a half dozen alert and well trained minds had never happened to be biased by the study of anv outline of history and had by some happy and incredible fortune never perused a "standard" historical work. Let us suppose that they had nevertheless learned a good deal about the past of mankind directly from the vast range of sources that we now possess, both literary and archæological. Lastly, let us assume that they were all called upon to prepare independently a so-called general history, suitable for use in the higher schools. They would speedily discover that there was no single obvious rule for determining what should be included in their review of the past. Having no tradition to guide them, each would select what he deemed most important for the young to know of the past. Writing in the twentieth century, they would all be deeply influenced by the interests and problems of the day. Battles and sieges and the courts of kings would scarcely appeal to them. Probably it would occur to none of them to mention the battle of Issus, the Samnite wars, the siege of Numantia by the Romans, the advent of Hadrian, the Italian enterprises of Otto I., the six wives of Henry VIII. or the invasion of Holland by Louis XIV. It is tolerably safe to assume that none of these events, which are recorded in practically all of our manuals to-day, would be considered by any one of our writers as he thought over all that man had done, and thought, and suffered, and dreamed, through thousands of years. All of them would agree that what men had known of the world in which they lived, or had thought to be their duty, or what they made with their hands, or the nature and style of their buildings, public and private, would any of them be far more valuable to rehearse than the names of their rulers and the conflicts in which they engaged. Each writer would accordingly go his own way. He would look back on the past for explanations of what he found most interesting in the present and would endeavor to place his readers in a position to participate intelligently in the life of their own time. The six manuals when completed would not only differ greatly from one another but would

have little resemblance to the *fable convenue* which is currently accepted as embodying the elements of history.

History in its broadest sense, is, in short, nothing less than the experiences of our race, so far as we can determine or surmise them. And what uses are we to make of the experiences of the race? The same kind of use that we make of our own individual history. We may question it as we question our memory of our personal acts, situations and past ideals. But those things that we recall from the superabundant fund of our own experiences vary continually with our moods and preoccupations. We instinctively adjust our recollections to our immediate needs and aspirations and ask from the past light on the particular problems that face us. Just as our individual history is thus not immutable but owes its value to its adaptability, so with the history of mankind. As Maeterlinck has beautifully said, with increased insight, "historic facts which seem to be graven forever on the stone and bronze of the past will assume an entirely different aspect, will return to life and leap into movement, bringing vaster and more courageous counsels." History is then not fixed and reducible to outlines and formulas but it is ever alive and ever changing, and it will, if we will but permit it, illuminate and explain our lives as nothing else can do. For our lives, are made up almost altogether of the past and each age should be free to select from the annals of the past those matters which have a bearing on the matters it has specially at heart.

If we test our personal knowledge of history by its usefulness to us, in giving us a better grasp on the present and a clearer notion of our place in the development of mankind, we shall perceive forthwith that a great part of what we have learned from historical works has entirely escaped our memory, for the simple reason that we have never had the least excuse for recollecting it. The career of Ethelred the Unready, the battle of Poitiers, and the negotiations leading up to the treaty of Nimwegen are for most of us forgotten formulæ, no more helpful, except in a remote contingency, than the logarithm of the number 57. The ideal history for each of us would be those facts of past human experience to which we should have recourse oftenest to our endeavors to understand ourselves and our fellows. No one account would meet the needs of all, but all would agree

that much of what now passes for the elements of history meet the needs of none.

It would take too long to attempt an analysis of the value of a genetic treatment of the elements in our social life. It is perhaps the greatest single discovery of modern times that we understand a situation best through its history, and this discovery has revolutionized every branch of organic and social science. Indeed we ordinarily first get a fairly comprehensive notion of a given phenomenon by tracing its origin and development, whether it be the human backbone, the order of St. Benedict, the stock exchange, the Wagnerian opera, or the doctrine of *stare decisis*. In many cases the knowledge of the history of an institution not uncommonly gravely affects our attitude toward it. The United States Senate looks different to one familiar with the history of the bicameral system and to one who is not. The Puritan sabbath could never have sustained a critical historical examination. One's views of democracy, or of the present laws of property, or of the prevailing economic organization, can readily be deeply affected by a study of the earlier conditions which lie back of present conditions. History has a disintegrating effect on current prejudices which is as yet scarcely appreciated. It makes both for understanding and for intellectual emancipation as nothing else can.

Obviously history must be rewritten, or rather, innumerable current issues must be given their neglected historic background. Our present so-called histories do not ordinarily answer the questions we would naturally and insistently put to them. When we contemplate the strong demand that women are making for the right to vote, we ask ourselves how did the men win the vote? The historians we consult have scarcely asked themselves that question and so do not answer it. We ask how did our courts come to control legislation in the exceptional and extraordinary manner they do? We look in vain in most histories for a reply. No one questions the inalienable right of the historian to interest himself in any phase of the past that he chooses. It is only to be wished that a greater number of historians had greater skill in hitting upon those phases of the past which serve us best in understanding the most vital problems of the present.

COLUMBIA UNIVERSITY.

THE ATOMIC WEIGHT OF VANADIUM DETERMINED FROM THE LABORATORY WORK OF EIGHTY YEARS.

By DR. GUSTAVUS D. HINRICHS.

(*Read April 21, 1911.*)

Vanadium can no longer be considered a rare element. Ferro-vanadium is produced on a large scale for the manufacture of special vanadium steels. Strangely enough, it was in a kind of natural vanadium iron that Sefström detected this element eighty years ago.

In 1830, while technical director of the famous iron works at Taberg in Smaland, Sweden, Sefström thought it might be interesting to submit his high quality malleable iron to the Rieman Test for cold-short iron, notwithstanding the apparent absurdity of such an undertaking. Accordingly he took one of his bars. On a part of its bright metallic surface he drew the little circular ridge of tallow and poured dilute sulphuric acid into the shallow dish thus formed, expecting, of course, to see no change whatever of the bright metallic bottom of this improvised dish. But he was amazed to see that bright bottom instantly turn black while the shallow dish rapidly filled up with a black powder, exactly as it does when the iron tested is badly cold-short.

The distinguished disciple of the great Berzelius instantly realized that this striking contradiction between test and fact was a positive indication of the presence of a hitherto unknown chemical element. Accordingly he set about isolating this new element. Working up quite a number of pounds of his iron, Sefström obtained less than a decigramme of the substance from which the new element was to be separated. Hence he turned hopefully from the iron to its fresh slag and found it to yield a much larger per cent. of the black powder. He now soon succeeded in isolating the new element

which, as a good Scandinavian, he named vanadin after Vanadiis, a designation of Freya, the greatest Goddess in Valhalla.

Sefström had promptly informed his teacher of the discovery and soon after brought his entire stock of the new element to Berzelius, requesting him to continue the research for which his own industrial work and the professorial duties at the Fahlun Montan-School left him neither the leisure nor the facilities. For a short time Sefström worked with Berzelius on the new element in that famous "Kitchen Laboratory" where Berzelius alone completed the splendid work of which he published a summary on pp. 99–110 of the "Annual Report" which he presented to the Swedish Academy of Sciences on March 31, 1831—exactly eighty years ago.

For almost forty years the element vanadin of Sefström and Berzelius remained undecomposed, but the striking isomorphism of the mineral vanadinite with the remarkable isomorphic group of apatite and pyromorphite presented the anomalous condition of the isomorphism of the element vanadin of Berzelius with the group PO of apatite and pyromorphite. This anomaly invited further attempts of the reduction of vanadin in which Roscoe was successful, 1867, proving vanadin to be really the oxide VaO, in which Va is the symbol of the present element vanadium of the atomic weight 51. This fully explains the isomorphism of vanadinite containing the oxide VaO, with pyromorphite, containing the corresponding oxide PO.

In this first research of Berzelius on vanadium, the old master already determined the atomic weight of the new element; for his value 67 for what we now know to have been VaO gives Va 51. He devised and used five distinct chemical methods for this atomic weight determination to which not one new method has been added in the eighty years elapsed since that work was done by the great chemist in his kitchen laboratory. It is a well-authenticated historic fact, Berzelius not only made atomic weight determinations for vanadium, but they were as accurate as those made forty years later by Roscoe, while some were as precise as corresponding determinations made eighty years later by Prandtl; besides, not only Roscoe and Prandtl, but all chemists have done this work by means

of the methods devised by Berzelius which he practiced in his laboratory in 1831.

It is therefore with great astonishment that I read in the first edition of the "Recalculation" of F. W. Clarke: "Roscoe's determination of the atomic weight of vanadium was the first to have any scientific value. The results obtained by Berzelius . . . were unquestionably too high, the error being probably due to the presence of phosphoric acid in the vanadic acid employed."

The same erroneous statement is repeated identically at the opening of the chapter on vanadium in the succeeding two editions of the work as may be seen by comparing: p. 183, edition 1882; p. 211, edition 1897, and p. 305, edition 1910.

The only new method, quite recently applied to the determination of the atomic weight of vanadium, is that of Edgar F. Smith.[1] This admirable method strictly conforms to the Berzelian advice "to chose such chemical methods for atomic weight determinations that the final result shall depend as little as possible on the operator's skill in manipulation." In my summary of the work of one hundred years on the determination of the atomic weight of hydrogen[2] I have given this great rule of Berzelius, in his own handwriting, from his "Sjelfbiografiska Anteckningar," published by the Kgl. Svenska Vetenskapsakademien, 1901, p. 41.

This rule requires to select such chemical reactions in which the physical and chemical characters of the substances weighed are so definitely fixed that the unavoidable errors of man and his instruments become negligible quantities. Such is the reaction no. 311 above referred to. Hence the work done by McAdam in the laboratory and under the direction of Edgar F. Smith has furnished the highest direct chemical approximation obtainable to the absolute scientific truth that Va is 51 exactly. This will appear, we think, from a careful examination of all the results actually obtained during the eighty years from 1831 to 1911 as plotted in our two diagrams no. 730 and no. 731 published with this paper.

The above reference to the presence of phosphoric acid in the

[1] See *Journal Amer. Chem. Society*, 1910, p. 1603, in the December number.
[2] In the *Révue Generale de Chimie*, 1910, Nos. 22 and 24.

vanadic acid used by Berzelius reminds us of the homely but sound scriptural advice habitually given by Berzelius to his disciples: "do not strain at a gnat while swallowing camels." The phosphoric acid in the vanadic acid used by Berzelius was detected by Roscoe in the sample which Berzelius had presented to Faraday; but the molybdic reagent necessary for the detection was not known to chemistry in the year 1831 when Berzelius did his work on vanadium.

As a matter of fact, Berzelius did not see this gnat; but his work shows that he did avoid some of the camels that stalk about the laboratories and which were deglutinated unconsciously forty and eighty years after Berzelius failed to strain that gnat. The error-shares due to the oxygen are the fattest and most numerous of these camels, up to the present day.

Our Method of Reduction.

In order to solve the riddle of the conflicting experimental data obtained in the chemical laboratories of the world during an entire century of painstaking work, we have, especially in the last quarter century, carried on special researches on the proper mathematical reduction of this kind of laboratory work.

The final results of this extended research are briefly summarized in five tables of which two only have thus far been published. Our work itself has been published in the following books and special papers:

"The True Atomic Weights," St. Louis, 1894, xvi + 256 pp., 8vo, with 7 plates and many illustrations. Dedicated to Berthelot.

"The Absolute Atomic Weights," St. Louis, 1901, xvi + 304 pp., 8vo, with portrait of Berzelius and three plates.

"The Proximate Constituents of the Chemical Elements," St. Louis, 1904, with 7 portraits, many plates, 112 pp. text, 8vo. This is an inductive treatise of the subject.

The "Cinquantenaire," 1910, gives some historical data, copies of older papers, letters in fac-simile and "Fragments inédits" with fine diagrams; 66 pp., 4to, with plates and portraits.

"Notes" published in the *Comptes Rendus* of the Academy of Sciences of Paris from 1873 to the present, almost sixty in number,

forming a volume of over 200 pp. 4to. The first, and in fact the greater number of the "Notes," were presented by Berthelot; others were presented by Messrs. Gautier, Lemoine, Haller, Gernez, and other academicians.

In the *Moniteur Scientifique,* from 1906 to 1909 more than a dozen longer articles have appeared with many diagrams. The first two tables above referred to are found in the November number for 1901, with discussions, pp. 731–744.

The papers were originally written in four languages: Danish, German, English and French. To these papers the reader may be referred by the Cinquantenaire and the list in the Prox. Constit. The results obtained, being in conflict with the dominant chemical school, have not been widely circulated except as adopted children.

For these reasons it is necessary here to give enough of the details of the finally worked out practical method of reduction to enable the reader to repeat all the calculations required, so that he can verify the results given.

It will then be seen that the final method is quite simple; the difficulty was to get this method.

Let α represent *the absolute atomic weight* of any chemical element, that is the whole or round number ($\frac{1}{2}$, or even $\frac{1}{4}$) which the experiments indicate to be near the *true atomic weight A,* which exactly to determine is the object of the reduction. The *unit* adopted is exactly $\frac{1}{12}$ of carbon-diamond which is practically identical with $\frac{1}{16}$ of that of oxygen.[3]

The *departure* of the true atomic weight from the absolute atomic weight we designate by the Greek letter epsilon (ϵ); that is: $A = \alpha + \epsilon$. This departure, as a matter of fact, is found to be a small fraction of the unit; we invariably express it in thousandths of that unit.

This departure—in units of the third decimal—is really our new variable, the quantity to be determined. This apparently insignificant matter of form is really of the greatest importance. For this new variable all products and powers become negligible quantities

[3] *Comptes Rendus,* 117, p. 1075, 1893.

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in our necessary calculations, because the departures are small quantities; hence, *all calculations,* even involving the most complex mathematical functions, are reduced to the simple rule of three and *carried out by proportional parts.* The importance will soon be recognized by the practice of the method.

The actual *laboratory work* consists essentially in the determination of *two weights* which we denote by p and q and which represent chemically pure compounds of the formula P and Q respectively. The necessary condition is that the weight p has been completely changed into q according to the exact formulæ P and Q by means of a suitable *chemical reaction.* Of such reactions we have tabulated and examined over three hundred that have been actually used for atomic weight determinations. We designate each such reaction by a number for ready reference. This number is simply marking their place in our table above referred to; it is arbitrary but a practical necessity. We have already above referred to the remarkable chemical reaction, recently applied in the Harrison Laboratory of the University of Pennsylvania as reaction no. 311.

Substituting the absolute atomic weights a for the chemical symbols in the formulæ of the two compounds P and Q, we can readily calculate the value of the quotient P/Q which we call the *atomic ratio* R and calculate the same to five decimals, the limit of precision today. On the following pages, giving the data for the chemical reactions that have been used for the determination of the atomic weight of vanadium there will be found examples of these and of all other processes, to which we request the reader to turn as new operations are defined.

On the other hand, the weights actually taken in the laboratory and designated by the letters p and q will give the *analytical ratio* r which we calculate also to five decimal places. The analytical ratios determined by the different experiments with the same two compounds P and Q will give hardly any identical values of r; we notice their *extreme values,* that is the maximum and the minimum in any given series of determinations made in the same manner with the identical material. The differance between the greatest and the least value of the analytical ratios of a series is the range of that series. This characterizes the *concordance* of the different deter-

minations of any series without introducing any false theoretical notion, as is done by the calculation of the so-called probable error of the mean. The actual *mean value* we do calculate and use.

While the individual analytical ratios vary for the different individual determinations in a series and even the means for the different series, it is found, as a matter of fact, that they bear a close relation to the atomic ratio. We call the excess of the analytical ratio over the atomic ratio, the *analytical excess* and designate it by the symbol e. That is: $r = R + e$. The value of e is also expressed in units of the fifth decimal.

THE EQUATION OF CONDITION AND THE SOLUTION EX-ÆQUO.

In the true atomic weights of 1894 (p. 139 to p. 169, esp. p. 158) the solution of the great problem is already shown to require an application of the method of the variation of constants.

In the absolute atomic weights of 1901, the change or variation Δ of the atomic ratio for an increase of 0.1 in the atomic weight is determined for each reaction and applied for several important objects throughout the entire work. On pages 144–147 of that work the final solution is really given but implicit only, and lacking the equal distribution of the analytical excess among the elements present in the reaction.

The actual equation of condition was established in 1907 through long and difficult work, both analytical and geometrical. The general analytical deduction by means of Taylor's formula was in the hands of eminent men abroad in the form shown in fac-simile (reduced to ⅓) as printed p. 61 of my "Cinquantenaire," 1910. The most general construction, which permits the establishment of a criterion for the absolute atomic weight, is printed on p. 60 of the same "Cinquantenaire."

Here we will present the final practical solution of the resulting "insoluble" indeterminate or diophantic equation in the simplest and most direct manner, suitable for common, current, practical application.

The *true atomic weight, A,* is the quantity sought, in the unit for which carbon-diamond is 12 exactly.

The *absolute atomic weight* α is indicated by the laboratory work; in case of doubt, the criterion just referred to has to be made use of.

The *departure* ϵ is expressed in units of the third decimal (thousandths) of the unit of atomic weights. Its exact determination is the main object of this paper.

The *atomic ratio* R is a function of the absolute atomic weights, expressed by the quotient P/Q above given.

If now the absolute atomic weight for any one given element in this ratio be increased by 0.1, that ratio will change or vary by an amount readily calculated from the formula of R as given; we use throughout seven place logarithms which give the precise value sought with the least trouble. This change or *variation* we denote by the Greek *capital delta* Δ for the particular element of which the atomic weight was increased by 0.1 in the atomic ratio. In 1901 we made this calculation only for one element in the ratio; now it has to be made for every element in the ratio.

In the tables here following this matter will become quite readily understood by simply repeating some of the calculations thus indicated. For the reaction no. 98 this work is quite simple, for only two elements are present, namely Va and O. In reaction 270, the work required is about double in amount, because four elements are in reaction, namely: Va, O, Cl and Ag.

The *analytical ratio* r has to be calculated for each single determination made; it is considerably simplified if the weighings are given by the chemist to the hundredth of the milligram, are rounded off to the tenth of the milligram, which is as far as the weighings can be trusted; see, for example, my demonstration of this fact for the weighings of Richards made at Harvard-Berlin.[4]

The *analytical excess* e is now obtained as it is $r-R$; it is also expressed in units of the fifth place.

Now we have in hand all the quantities required for obtaining *the departure* ϵ sought, by solving the equation of condition. While this indeterminate or diophantic equation is, of course, insoluble in general, we have nevertheless obtained two practical solutions of the same[5] of which the one properly named *ex-æquo* is the most

[4] *Moniteur Scientifique*, Juin, 1909, especially pp. 384–385.

[5] *Comptes Rendus*, T. 149, p. 1074, 1909, with a most instructive figure.

serviceable and by far the most readily understood and easiest applied.

Our general deduction (really as indicated 1894 already: a method of the variation of the constants) leads to the simple form of the *equation of condition*

$$100\ e = \Sigma \Delta\epsilon,$$

where the constant 100 presupposes that the analytical excess e and the variation Δ are expressed in units of the fifth place while the departure ϵ is expressed in units of the third place or thousandths of the unit of atomic weights.

It may not be amiss here to insist on the fact that since in every chemical reaction there are at least two elements present, the above equation contains at least two unknown departures ϵ and is therefore really an indeterminate or a diophantic equation.

Our practical solution ex æquo of this equation is as follows: Let m be the number of elements involved in the chemical reaction used, then the number of terms $\Delta\epsilon$ in the above sum Σ is m.

Ascribing to all elements an equal influence on the error or excess e, the part thereof due to each element will be $e' = e/m$.

Hence the actual departure ϵ for each element in the reaction will be determined by the simple relation

$$\epsilon = \frac{100\ e'}{\Delta}.$$

If the value of Δ be above a certain limit, this determination will be sharp; the corresponding reaction therefore may also be called *sharp*.

But if the value of the variation Δ for any element is small, the *reaction* for that element *will be dull* and the determination of the atomic weight will be impossible with any high degree of precision, as we have shown in *Comptes Rendus*, T. 148, p. 484, 1909, in the attempted determination of the atomic weight of Tellurium by a reaction quite dull for that element.

This one attempt strikingly shows the real condition of the work of the dominant school to be irrational.

After having briefly explained the manner in which we have tried to solve the great problem of the deduction of the true atomic

weights from the experimental work done in the laboratories, we may proceed to the full statement of the facts obtained for the element vanadium during the past eighty years and the final results of our discussion of the same.

We shall present the facts in the most compact form of tables and finally exhibit them to the eye in the form of accurately drawn graphics, from which we shall be able to read the final result the most readily and clearly.

THE ACTUAL DETERMINATION OF THE TRUE ATOMIC WEIGHT OF VANADIUM.

I.—Absolute Atomic Data.

Fundamental Constants, Calculated from the Absolute Atomic Weights.

Only seven[5a] chemical reactions have been used for the determination of the atomic weight of vanadium, thus far; they are the following:

No. 98: Pentoxide reduced by hydrogen.

No. 269: Oxychloride to silver.

No. 270: Oxychloride to silver chloride.

No. 311: Vanadate to chloride.

(*a*)—Oxychloride to pentoxide.

(*b*)—Sulphate to barium sulphate.

(*c*)—Sulphate to pentoxide.

The last three preliminary methods of Berzelius have been used by him, each once only, and by no other chemist, except that Roscoe made four determinations according to method (*a*). No. 311 has but just been introduced by Edgar F. Smith, December, 1910. All the chemical reactions used for the determination of the atomic weight of vanadium, up to that date, were devised and first used by Berzelius in 1831, eighty years ago. It seems that his work has some scientific value, after all.

In the following Table I. we have given the most important fundamental constants required by our method of reduction. They have all been calculated from the well-known absolute atomic weights: Va, 51; T, 16; Cl, 35½; Ag, 108; Na, 23; S, 32; Ba,

[5a] If we count 269 and 270 as distinct reactions.

137½; H, 1.008 instead of the exact value 1.00781 determined atomechanically by us (*Révue gén. de Chimie*, 1910, p. 386).

TABLE I.

FUNDAMENTAL CONSTANTS FOR VANADIUM.

No.	Formula.	Atomic Ratio, R.		Variation Δ for			
		Fraction.	Decimal.[e]	Va.	O.	Cl.	Metal.
98	$\frac{O_2}{Va_2O_5}$	$\frac{32}{182}$	0.17 582	—19	62	—	—
269	$\frac{VaOCl_3}{3Ag}$	$\frac{173.5}{324.0}$	0.53 549	31	31	93	—49(Ag)
270	$\frac{VaOCl_3}{3AgCl}$	$\frac{173.5}{430.5}$	0.40 302	23	23	42	—28(Ag)
311	$\frac{NaCl}{NaVaO_3}$	$\frac{58.5}{122.0}$	0.47 951	—39	—118	304	43(Na)
a	$\frac{Va_2O_5}{2VaOCl_3}$	$\frac{182}{347}$	0.52 450	27	113	— 91	—
b	$\frac{Va_2O_5}{2BaSO_4}$	$\frac{182}{467}$	0.38 972	43	40	— 17(S)	—17 (Ba)
c	$\frac{Va_2O_5}{\text{Va sulphate}^f}$	$\frac{182.00}{398.06}$	0.45 722	27	—35	— 23(S)	—140(H)

II.—GENERAL SUMMARY OF THE EXPERIMENTAL WORK DONE.

In the common reviews of the experimental work done for atomic weight determinations, the amount of substance taken in each experiment is not made the subject of special consideration. This neglect is due to the erroneous estimation in which the so-called "*probable error*" of the mean is held.

This probable error has caused the most serious errors in all branches of physical science where it has been applied—in the unfortunately common way, without proper understanding. We have treated of this repeatedly, especially in our "absolute atomic weights, 1901, on the first hundred pages, to which we must refer.

In the language of Berzelius already quoted we might say the above probable error is the gnat strained at which hides from sight the camel-like systematic and constant errors which are swallowed. We have, at last, seen one admission of the fact we have always

[e] Between the second and third decimal of the five, we always leave a space to make the constancy of the first two conspicuous.

[f] The crystallized sulphate is $[VaO]_2S_2O_8 + 4H_2O$.

accentuated, that large constant and systematic errors may exist though the value found for the probable error of the mean is insignificant. In Clarke's third edition (1910, pp. 93–98) the probable error of the mean amounts to only one unit in the fifth decimal of the analytical ratio while the constant error of that ratio amounts to 120 such units, according to the famous analyses of Stas and Baxter.

To detect the constant and systematic errors we have always plotted the results of the individual experimental determinations as ordinates to the weight taken as abscissæ. In these diagrams the scale selected for the atomic weights or the ratios must be very great while that for the weight taken has to be small. In my diagram representing in this manner all the atomic weight determinations of hydrogen made in a century (*Révue gén. de Chimie*, 1910, p. 380) the unit of atomic weights is 30 meters (about 100 feet) while a decagram of water produced is represented by three centimeters (or a little over one inch).

To permit this *search for the really important constant and systematic errors*, we give the weight taken (to the decigram) in all our tables. For a series of determinations, we give the total weight taken for all the determinations of the series, and the mean weight taken for each determination—which is obtained from the total by dividing this latter by the number of determinations made.

Table II. thus shows that 50 determinations have actually been made for the determination of the atomic weight of vanadium on 4 grams each of the substance taken, not counting the seven preliminary determinations on 1 gram of matter each.

It is also seen at a glance that by reaction 98 the work of Roscoe ought to be the most reliable, while for 270 the work of Prandtl should be the best and that the work done by one chemist for 311 has been carried out under equally as favorable condition in regard to the weight operated upon.

By means of the reference letter specified in the last column the corresponding line on the diagrams can be instantly identified. We may here already remark, that the length of this line, extending to the right or to the left from the vertical in the middle, marks the magnitude of the departures for the elements as indicated by the chemical symbol added.

Thus the least deviation or departure has resulted from the use of the Method 311 recently introduced by Edgar F. Smith. The preliminary work of Berzelius, done eighty years ago, according to methods *a* and *b* as represented by lines *D* and *T* on the diagram, showed departures for sulphur and barium extending beyond the limit of our diagram: 470 to the left (negative) in line D and 776 to the right (positive) in line *T*; the former is almost half a unit, the latter three quarters of a unit of atomic weight.

TABLE II.

SUMMARY OF THE EXPERIMENTAL DETERMINATION.

Reaction.	Number of Determ.	Weight Taken, Grammes.		Chemist.	Letter on Diagram.
		Mean.	Total.		
98	3	1.6	4.7	Berzelius	B
	1	0.6	0.6	Berzelius	B*
	5	6.0	30.0	Roscoe	F
	4	2.5	10.0	Prandtl	C
	13	3.4	45.3		
269	9	2.7	24.3	Roscoe	N(M, R)
270	1	1.6	1.6	Berzelius	G
	6	1.2	7.4	Roscoe (A)	H } I.
	2	2.4	4.7	Roscoe (B)	K }
	5	4.7	23.5	Prandtl, I.	L
	6	4.8	28.8	" II.	P
	4	8.1	32.4	" III.	O
	24	4.1	98.4		
311	5	6.0	30.2	Smith-McAdam	Q
Total	51	3.9	198.2		
or say:	50	4	200		

PRELIMINARY REACTIONS:

(*a*) Oxychloride	1	1.6	1.6	Berzelius, 1831	A
to Pentoxide	4	1.0	4.0	Roscoe, 1868	E
(*b*) Va Sulphate to Ba Sulphate	1	0.8	0.8	Berzelius, 1831	D
(*c*) Va Sulphate to Pentoxide	1	0.8	0.8	Berzelius, 1831	T
Mean	7	1.0	7.2		

III.—The Analytical Results.

The chemical work, beginning with the determination of the weight p and ending with the determination of the weight q (or the inverse) gives directly the atomic ratio r, by a simple division carried to five decimal places. A simple substraction now will give the analytical excess e by using the atomic ratio calculated once for all for the reaction. The results obtained in this way are given in Table III.

We will here only call attention to the following peculiarly interesting circumstances:

"Analyst B" working under Roscoe makes three determinations under reaction 269, and takes almost the same weight of the oxychloride for each one of these determinations; notwithstanding the fact that he really only repeats one and the same determination three times, his results range over 501 units in the fifth place—an enormous range under so favorable conditions.

In the language of Berzelius, this enormous range is quite a big camel which was swallowed without an effort in the laboratory of Roscoe, who strained laboriously at the tiny gnat of phosphoric acid which a test of high delicacy, that was not yet known in the days when Berzelius did his splendid pioneer work on vanadium and courteously presented to Faraday a sample of the vanadic acid he had received from his disciple who discovered the element vanadium and which acid Berzelius had purified himself as far as the science of his time permitted. These facts I gather from Becker, Smithson. Misc. Collections, 358, Washington, 1880, p. 132, quoting from *Liebig's Annalen*, 93, p. 6, 1868.

TABLE III.

The Analytical Ratios Determined.

Reaction 98.—Atomic Ratio, $R = 0.17582$.

Berzelius, 1831.—Meyer-Seubert, "Atomgew.," 1883, p. 28.

Determ.	Sums of Weights. Pentoxide.	Oxygen.	r.		e.
3	4.6995	0.8120	0.17	278	—304
1	0.6499	0.1124		294	—288

Diagram. Lines B and B^x.

Roscoe, 1868.—Jour. Chem. Soc., 6, p. 330, 1868.

No.	1.	2.	3.	4	5.	Mean
Weight Oxide	7.7	6.6	5.2	5.1	5.4	6.0
$r = 0.17$	533	507	489	515	501	509
$e =$	— 49	— 75	— 93	— 67	— 81	— 73

Range 44.—Diagram, Line F.

Prandtl, 1910.—Jour. Am. Chem. Soc., 1911, pp. 266–7, from *Ztsch. anorg. Chem.*, 67, 257.

No.	1.	2.	3.	4.	Mean.
Weight Oxide	9.1	9.9	8.7	12.3	10.0
$r = 0.17$	261	376	395	394	356
$e =$	— 321	— 206	— 187	— 188	— 225

Range 133.—Diagram, Line C.

REACTION *269*.—Atomic Ratio, $R = 0.53549$.

Roscoe, 1868; Analyst A.—*Jour. Chem. Soc.*, Vol. 6.

No.	1.	2.	3.	4.	5.	6.	Mean.
Oxychlor.	2.4	4.7	4.2	4.0	0.9	1.4	2.9
$r = 0.53$	425	528	533	510	530	532	510
$e =$	— 124	— 21	— 16	— 39	— 19	— 17	— 39

Range: 108.—Diagram: Line M.

Roscoe, 1868; Analyst B.

No.	7.	8.	9.	Mean.	Mean of All 9.
Oxychlor.	2.9	2.1	1.4	2.1	2.7
$r = 0.53$	980	755	479	738	586
$e =$	431	206	— 70	189	37

Range: 501.—Diagram: Line R. Range: 555.

REACTION *270*.—Atomic Ratio, $R = 0.40302$.

Berzelius, 1831.—1 determ.: 1.6385 oxychlor. gave 4.0515 Ag, hence $r = 0.40442$ and e 140. Meyer-Seubert, "Atomgew.," 1882, pp. 90–91.

Diagram: Line G.

Roscoe, 1868; Analyst A:

No.	1.	2.	3.	4.	5.	6.	Mean.
Oxychlor.	1.9	0.7	0.8	1.4	1.0	1.6	1.2
$r = 0.40$	323	531	537	337	399	174	383
$e =$	21	229	235	35	97	— 128	81

Range: 363.—Diagram: Line H.

ANALYST B:

No.	7.	8.	Mean.	Mean of A and B.
Oxychlor.	2.2	2.5	2.4	1.5
$r = 0.40$	391	333	362	378
$e =$	89	31	60	76

Range: 58.—Diagram Line K. Diagram: I.

Prandtl and Bleyer.[8] Series I., 1909.

No.	1.	2.	3.	4.	5.	Mean.
Oxychlor.	5.5	5.9	3.2	5.3	3.6	4.7
$r = 0.40$	393	346	365	322	367	359
$e =$	91	44	63	20	65	57

Range: 71.—Diagram: Line L.

[8] Clarke, "Recalc.," 1910, p. 307 for I. and II.; *Journ. Am. Chem. Soc.*, 1911, p. 266, for III.

Series II., 1909.

No.	1.	2.	3.	4.	5.	6.	Mean.
Oxychlor.	4.9	3.7	5.0	6.5	4.3	4.1	4.8
$r = 0.40$	331	286	318	315	308	325	314
$e =$	29	— 16	16	13	6	23	12

Range: 45.—Diagram: Line P.

Series III., 1910.

No.	1.	2.	3.	4.	Mean.
Oxychlor.	7.8	8.4	10.7	5.5	8.1
$r = 0.40$	301	311	321	333	317
$e =$	— 1	9	19	31	14

Range: 32.—Diagram: Line O.

Reaction No. 311.—Atomic Ratio, $R = 0.47951$.

Edgar F. Smith and *McAdam, Jour. Am. Chem. Soc.*, 1910, p. 1614.

No.	1.	2.	3.	4.	5.	Mean.
Vanadate	4.9	5.6	4.4	5.8	9.5	6.04
$r = 0.47$	931	927	941	937	921	931
$e =$	— 20	— 24	— 10	— 14	— 30	— 20

Range: 20.—Diagram: Line Q.

Strictly this series consists of 3 determinations only, two of which have been made twice, as follows:

No.	1.3.	2.4.	5.	Mean.
Vanadate	4.6	5.7	9.5	6.0
$r = 0.47$	936	932	921	931
$e =$	— 15	— 19	— 30	— 20

Range: 15.

This shows how even within narrow limits of weights taken (here from 4½ to 9½ grammes) *the systematic error* becomes evident as a function of the weight taken. Within the actual range, the analytical excess approaches zero with diminishing amount operated upon.

Preliminary Work.

Line.	Reaction.			Analytical Ratio. r.	Excess. e.	
A.	a 1 det.	1.6385 oxychl. gave	0.874 oxide	0.53342	892	Berzelius.
E.	4 det.	4.0418	2.1258	0.52608	158	Roscoe.
D.	b 1 det.	0.351 pentoxide	0.913 Ba sulphate	0.38445	— 527	Berzelius.
T.	c 1 det.	0.351 pentoxide from	0.775 Va sulphate	0.45290	+ 432	Berzelius.

IV. The Analytical Excess.

The analytical excess e is only comparable in work carried out according to the same chemical reaction by different chemists. This condition has determined the form of Table IV., in which the capital letter marks the line on our final diagram representing the work, while the numbers below the letter represent: the first, the number of determinations made, the second giving the analytical excess obtained. This excess will naturally be found the greatest for all preliminary (or pioneer) work such as that done by Berzelius in his single trials of reactions (a), (b), (c).

This table makes it very apparent that each succeeding chemist benefited by the work and experience of his predecessor. This shows best under reaction 270 where Berzelius (1 determination) gives the excess 140, Roscoe (8 determinations) only 76 and Prandtl (15 determinations) brings it down to an average of 28 only.

For Reaction 98 this relation holds good for the work of Berzelius and Roscoe only, while that of Prandtl, done as recently as 1909, reaches almost the excess of Berzelius single first trial of eighty years ago, although Prandtl used the mean weight of 2.5 grams while Berzelius had only half a gram for his work.

TABLE IV.

The Resulting Analytical Excess.

Reaction.	98		269		270		311		*a*		*b*		*c*	
Chemist:														
Berzelius,	B				G				A		I		D	
1831	3	−304			1	140			1	892	1	−527	1	432
	B′													
	1	−288												
Roscoe,	F		R		H				E					
1868	5	− 73	3	189	6	81			4	158				
			N		I									
			9	37	8	76								
			M		K									
			1	−39	2	60								
Prandtl,	C				L									
1909	4	−255			5	57								
					O									
					4	14								
					P									
					6	12								
Smith-McAdam,		—					Q							
1910							5	−20						

V. The Final Departures.

In this table (IV.) we have finally given all the departures for all the elements taking part in the different reactions used for the determination of the atomic weight of vanadium during the last eighty years—from Berzelius in Stockholm to Edgar F. Smith in Philadelphia. We have fully explained the manner of calculating these departures; every reader can verify any of these values for himself by the methods stated. Since the element most concerned is vanadium, we have arranged the record of departures in the order of the magnitude of the departures of vanadium, for which the extreme values are 1062 and minus 307, giving a total range of 1369, say one and one third units of atomic weight; that is, from Va 52.06 to 50.69.

If we omit the pioneer and preliminary work done by Berzelius in trying reactions (*a*) and (*b*), the extreme departures will be 800 and —31, a total range of 831 only. The atomic weights will run from 51.80 to 50.69, which is a remarkably fine showing for so long a series of very difficult work on the rare element.

To realize the generally excellent work of the early days of Berzelius and of Roscoe—when the element was decidedly rare and difficult to purify—we need only compare the limits of the determinations by Prandtl of today with the range of the entire series (excluding only, as we have done already, the reactions (*a*) and (*b*)); these extremes of Prandtl are 594 with reaction 98 and 13 in Series II. with reaction 270; a total range of 581 thousandths. The corresponding atomic weights of vanadium are 51.59 and 51.01 differing 58 hundredths.

We are greatly tempted to point out a number of interesting features on this table, but fear that the paper will assume undue length and trust the reader will help himself.

We only remind the reader that the letter in the first column of this table permits the ready identification of the experimental result expressed in numbers in this table with the graphical representation on our two diagrams.

200.

TABLE V.

THE DEPARTURE. DETERMINED FROM EIGHTY YEARS OF WORK.

Line in Diagram.	Chemist.	Reaction.	No. of Determ.	Analyt. Excess. ϵ.	Departure, ϵ, in Thousandths.				True Atomic Weight	
					Va	O	Cl	Metal.	Va.	O.
A	Berzelius	*a*	1	892	1062	263	—326		52.06	16.26
B	"	98	3	—304	800	—245			51.80	15.75
B*	"	98	1	—288	784	—241			.78	.76
C	Prandtl	98	4	—225	594	—183			.59	.82
D	Berzelius	*c*	1	432	400	—310	—470[9]	— 77[10]	.40	.69
E	Roscoe	*a*	4	158	195	47	— 58		.20	16.47
F	"	98	5	— 73	192	— 59			.19	15.41
								Ag:		
G	Berzelius	270	1	140	153	153	83	—125	.15	16.15
M	Roscoe (B)	269	3	189	150	150	50	— 95	.15	.15
H	" (A)	270	6	81	87	87	47	— 71	.09	.09
I	" (A, B)	270	8	76	83	83	45	— 68	.08	.08
K	" (B)	270	2	60	65	65	36	— 54	.07	.07
L	Prandtl, I	270	5	57	62	62	34	— 51	.06	.06
N	Roscoe (M, R)	269	9	37	30	30	10	— 19	.03	.03
O	Prandtl, III	270	4	14	15	15	7	— 12	.01	.01
P	" II	270	6	12	13	13	7	— 11	.01	.01
								Na:		
Q	Smith-McAdams	311	5	— 20	13	4	— 2	— 12	51.01	16.00
								Ag:		
R	Roscoe (A)	269	6	— 39	— 31	— 31	— 10	20	50.97	15.97
T	Berzelius	*b*	1	—527	—307	—330	776[9]	776[11]	.69	15.67

[9] S. [10] H. [11] Ba.

VI. OUR GRAPHICS.

The values of all departures given in Table V. are represented to the eye in our two graphics.

Fig. 1 gives all the larger departures and as many of the smaller ones as space would permit. The scale used is 200 thousandths of the unit of atomic weights to the inch; or, what amounts to the same, the unit of the atomic weights is represented by five inches in length.

Fig. 2 gives all the departures of the central region on a scale which is five times the one used in the construction of the first figure. Hence in Fig. 2 the unit is represented by a line of twenty-five inches; or, in other words, it shows forty thousandths to the inch.

The vertical of the ordinate represents the departures of vanadium, while the departures of the elements combined with vanadium are set off on the horizontal as abscissæ.

The results for a complete analysis of any given compound are therefore set off on the horizontal line drawn through the point on the vertical determined by the departure for vanadium.

200.

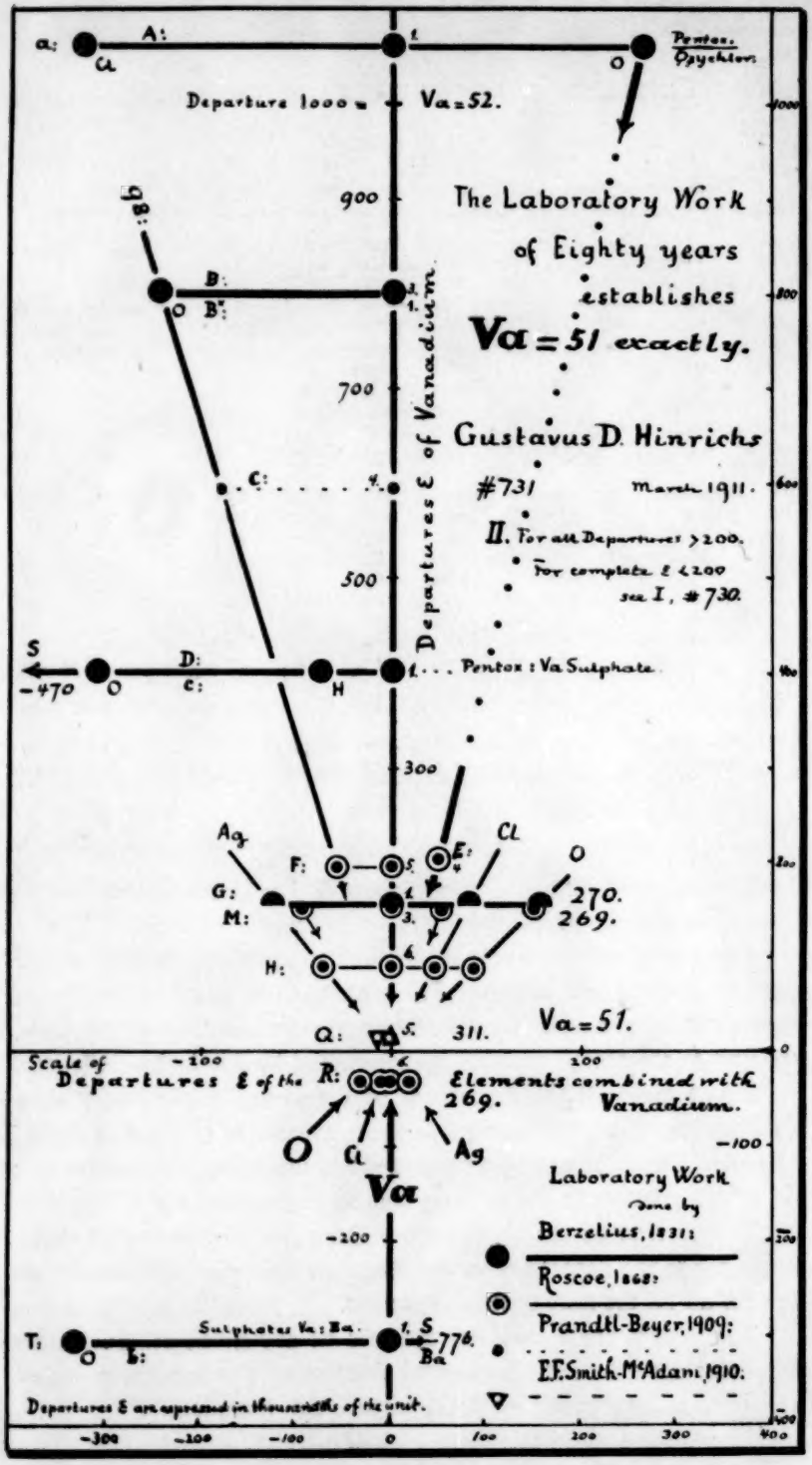

FIG. 1. Departures. Scale 200 thousandths to the inch.

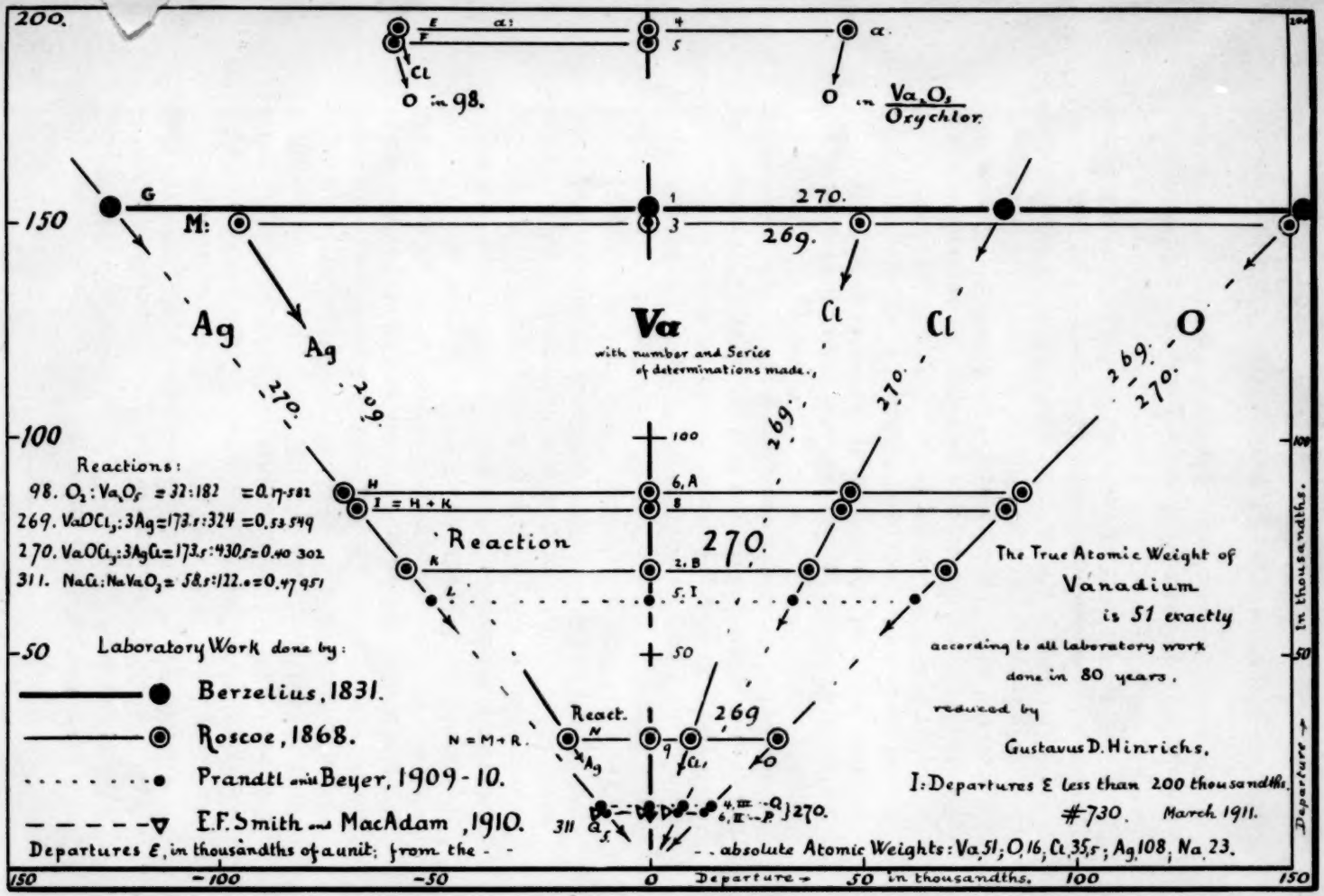
200.
150
100
50
0
-50
-100
-150
Departure → 50 in thousandths.
Departure → in thousandths.
O in 98.
O in Va2O5 / Oxychlor.
270.
269.
Ag
Ag
Va
with number and Series of determinations made.,
Cl
Cl
O
Reactions:
98. O2 : Va2O5 = 32:182 = 0.17582
269. VaOCl3 : 3Ag = 173.5 : 324 = 0.53549
270. VaOCl3 : 3AgCl = 173.5 : 430.5 = 0.40302
311. NaCl : NaVaO3 = 58.5 : 122.6 = 0.47951
Reaction 270.
The True Atomic Weight of Vanadium is 51 exactly according to all laboratory work done in 80 years, reduced by Gustavus D. Hinrichs.
Laboratory Work done by:
Berzelius, 1831.
Roscoe, 1868.
Prandtl and Beyer, 1909-10.
E.F. Smith and MacAdam, 1910.
N = M + R.
React.
269
270.
311
I: Departures E less than 200 thousandths.
#730. March 1911.
Departures E, in thousandths of a unit; from the - - absolute Atomic Weights: Va 51; O 16; Cl 35.5; Ag 108; Na 23.

FIG. 2. Departures, E. Scale: 40-thousandths to the inch.

The chemist whose work is represented is indicated by the special mark used to designate the point, as shown in the explanation of signs on the diagram.

The small figure near the vanadium sign on the vertical indicates the number of determinations made represented in that line.

The line itself is marked by a letter, used in the tables for the purpose of ready identification.

For each reaction, the geometrical place (or locus) is a straight line passing through the center or the origin; the angle under which it cuts the axes is determined by the ratio of the variation of the element concerned and that of vanadium. These lines can therefore be drawn before any laboratory work is done, depending entirely on the chemical formulæ of the compounds taken and obtained in the reaction used.

For further particulars, some of which are very interesting as well as useful, we may refer to page 60 of our "Cinquantenaire," where also a remarkable criterion is given, permitting to detect any error in the assumed absolute atomic weight. The example there taken is copper.

Our two figures here inserted bring into clearest possible view the fundamental fact that all these departures are co-related; that the experimental error is not thrown on the vanadium for which the atomic weight is sought, but is distributed ex-æquo to all elements partaking in the reaction, as we have shown in formulæ, but which is here presented to the eye directly.

We do not recognize or find the slightest pretext for the assumption that any one element is immaculate and cannot be conceived to partake in any error of whatever cause or origin; but we have found that all elements in a chemical reaction are affected by the same cause of error according to the ties that bind them and which we have read in the chemical formula and in the mathematical relations first studied by Lagrange under the name of the Variation of arbitrary constants.[12]

We know that it is absurd to suppose that oxygen is always found to be 16, absolutely unaffected by any error, physical or chemical, in practice; that next some other atomic weight of some other element

[12] "True Atomic Weights," 1894, p. 158.

can be determined and that this value also will remain unaffected in all reactions; and so forth. That errors rapidly accumulate in such an irrational process, we have shown as far back as 1893, almost twenty years ago. That paper was presented by Berthelot to the Academy of Sciences of Paris and published in its *Comptes Rendus.*[13]

After having completed a thorough examination of all the atomic weight determinations made, we have, by a sort of crucial test, demonstrated that the present value Ag 107.88, implying a departure of 120 thousandths, is impossible; that O 16 requires Ag 108 exactly, according to all determinations made during an entire century in all the laboratories of the world.[14]

It is this same principle that is demonstrated by the diagrams here printed and this demonstration is made visible to the eye: It is not vanadium alone that causes the error affecting the laboratory work, but all elements in the reaction contribute to the error recognized in the final result of the analytical work.

Instead of the common notion that the work of the different chemists conflicts in the different values they have presented as the results of their determination of the atomic weight of vanadium our figures here inserted show to the eye that *all determinations made agree in the common result of Va 51 exactly.* While no experimental work of any kind, done by man, with instruments and by chemical reactions, all of which are but approximations to a mathematical perfection, can be expected to give perfectly exact results, we have proved that the final error cannot be ascribed to vanadium alone, as continues to be done by the dominant school, but that on the contrary, all the elements present in a reaction contribute, each one its share, to the excess or deficiency resulting. It was therefore necessary to find the laws regulating this participation of the different elements in the errors of the reactions and of the entire experimental work. Having discovered these laws, we have applied them here, to the atomic weight determinations made for vanadium and present in the two graphics (Figs. 1 and 2) the final results thus obtained.

These figures show plainly that all the departures from the abso-

[13] T. 116, p. 695.

[14] See paper read December 2, 1910, before the American Philosophical Society; *Proceedings,* 1910, pp. 359–363.

lute values are converging to zero along each of the lines of work pursued in the eighty years by the different chemists; there is but one insignificant exception, which we shall consider, when we take up the recent work of Prandtl.

Our Figs. 1 and 2 proclaim that the atomic weight of vanadium is exactly 51 in all these determinations, just as sure as oxygen has the atomic weight 16 exactly and silver 108 exactly, chlorine $35\frac{1}{2}$ exactly, sodium 23 exactly; in fact, all the elements have as atomic weights exactly the absolute values given in our publications of the last twenty years.

Even the very first determinations made by Berzelius, with only a fraction of a gramme of material at service, and only in one single determination, by the reactions designated (*a*), (*b*) and (*c*), do confirm the value Va 51; for the deviation noted for Va affects all the other elements present as well, and therefore it would be absurd to suppose that the atomic weight of vanadium could be obtained from a reaction which fails to give an exact determination for the other elements present. Thus reaction (*c*) represented by line D in Fig. 1, gives by the single determination made by Berzelius on 8 decigrams of the rather complex hydrated vanadium sulphate, a departure of 400 thousandths from 51 for the atomic weight of vanadium; but the same determination gave the atomic weight of oxygen 310 thousandths low as marked on the figure; it also gave the atomic weight of sulphur 470 thousandths low as indicated near the edge of the diagram and by the arrowpoint: for the real circular mark falls far beyond the limit of the diagram.

Is it so hard to understand that a reaction that fails to give precise determinations for all the elements it involves cannot necessarily be expected to furnish a value of precision for vanadium? Is it not about time for each individual chemist to begin to consider these simple facts for himself, as was the practice in former days?

It would be interesting to trace the gradual approach to the center where all departures are zero, as exemplified in the actual work of the successive chemists. This will be found to hold good for all, with the single exception already mentioned. We will only point to a few special instances, expecting the reader to go over the entire ground by himself.

The preliminary analysis, represented in the line A at the top of Fig. 1, gave not only the greatest departure for Va, but also for O, in the only determination made by Berzelius. When Roscoe, forty years after, made a series of four determinations the result entered at *E* in our figure (both 1 and 2) he cut the departure from 1062 to 195 for Va and from 263 to 47 for oxygen. If this determination were repeated with the benefit of all the progress made in laboratory work, the resulting mark would undoubtedly find its place much nearer the zero departure on the line (mainly dotted) on our figure (1).

Let us also look at the results obtained by the use of reaction 270, best seen on Fig. 2, lines G, H, I, K, L, O, P. Of these lines, G is the most distant, representing the largest departures: it marks the one determination made by the old master in 1831. Then we meet, in going toward the center of perfection or zero departure, the lines H, I and K bearing the mark of Roscoe and only about half as far from the zero as the line G of 1831. The total number of determinations made by Roscoe in 1868 was 8, represented by line I; 6 of these eight were "done by Analyst A" and are represented by line H; the other two were "done by Analyst B" and are represented by line K. Finally we have three series (I., II. and III., represented by lines L, P and O, respectively) made by Prandtl quite recently; of these, the last two series come quite close to the center, the departure for Va being 14 only for the mean of the two series (see Table V.). Since on our diagram the departures for O, Cl and Ag are set off as abscissæ to that of Va taken as ordinate, *the gradual diminution of all the departures is strikingly shown in the lines for these elements converging to the point of zero departure.* At the same time we here have the positive evidence that Prandtl has produced two very concordant series (his II. and III.) with a very small departure (mean 14 for Va and O) and one series (his first, I.) represented by line L for which the departure is 62, that is more than four times as large.

Now we may consider the results obtained by the reduction test, reaction No. 98, represented on our Fig. 1 by the lines B, C and F. Here we meet that one exception before referred to; for the greatest departure (line B) of Berzelius is but slightly diminished by Prandtl's

quite recent work (line C) and far surpassed by the small departures of the much older work of Roscoe (line F). It is to be hoped that Prandtl will also make one or two additional series under reaction 98, as he has done under 270; we dare say that, with due care, he may repeat the experience he has reported for reaction 270 and greatly reduce the departure, at least to that of Roscoe in 1868.

It will hardly be necessary to state that the numerical data we have quoted in the discussion of our Figs. 1 and 2 are taken from tables II. and V. especially.

Conclusion.

We think that the reader will have no trouble now in completing his study of the facts placed before him in our tables and in our figures which both comprise much more than their size would seem to indicate.

We therefore think that the reader will fully understand the utter fallacy of throwing all the errors of all kinds on the one element, the atomic weight of which the modern chemist tries to determine "in the chemical laboratory and by experiment exclusively."

The reader will, we believe, now fully comprehend the situation—both of the chemist and of his intended victim, the element. The victim—if it were conscious—would shiver in anticipation of being made responsible for every error and mishap that may befall any of the elements present in the reactions, the apparatus used and even the chemist at work; for all these errors and shortcomings the modern chemical school *de facto* charges up to the element the atomic weight of which it undertakes to determine. The only new step—other than what the general progress of practical laboratory work may favor him with—will be the straining out a few more innocent gnats without in the least disturbing the ever attendant herd of the old camels.

It sometimes does seem strange that in twenty years this Berzelian picture from Saint Matthew (XXIII., 24) has remained so true to Nature. It is not the fault of the individual chemists, except in so far as they have surrendered a fundamental part of their rightful domain to the International Atomic Weight Committee.

THE ORIGIN AND SIGNIFICANCE OF THE PRIMITIVE NERVOUS SYSTEM.

By G. H. PARKER.

(*Read April 21, 1911.*)

Linnæus defined a plant as an organized, living, but non-sentient body and an animal as an organized, living, and sentient body. Although no modern biologist would attempt to support the contention that animals are sentient and plants are not, the distinction drawn by Linnæus is not without a certain foundation in truth, for sentience in its full development and as Linnæus probably understood it, is the exclusive and supreme possession of the higher animals. That these animals possess intelligence as contrasted with all other natural bodies is a statement to which few naturalists will offer any serious objection. The seat of this intelligence is the nervous system and, though the integrity of the other systems of organs is essential in most cases to the well-being of the animal body, the fact that the totality of activities that makes up the mental life of human beings as well as that of other animals, is absolutely dependent upon the nervous system, is evidence sufficient of the paramount importance of these organs. It is, therefore, not without interest to inquire into the origin of this system of organs and to trace the early steps by which it passed from a position of initial obscurity to one of the highest significance.

The nervous system of the higher animals, though enormously complex in its organization, is composed of relatively simple elements, the neurones, arranged upon a comparatively uniform plan. This plan is well exemplified in the spinal cord of the vertebrates. In this organ the sensory neurones, whose cell-bodies lie in the dorsal ganglia, extend from the integument through the dorsal roots to the gray matter of the cord. Motor neurones, whose cell-bodies are situated within the gray matter of the cord, reach from this region to the muscle-fibers which they control. These two classes

of neurones would seem to be sufficient for all ordinary reflex operations, but the cord contains within its limits many other neurones which serve to connect one part of its structure with another. These neurones, therefore, have been called association neurones, a term which has unfortunately proved to be somewhat misleading because of its use in psychology for quite a different range of phenomena. The so-called association neurones are interpolated between the sensory and motor elements just described and must thereby lengthen and extend the courses of the reflex impulses. Such neurones make up a large part of the substance of the cord and doubtless increase enormously its internal connections. In the brain they not only add to the nervous interrelations, but they afford in the region of the cerebral cortex the material basis for all intellectual operations.

The plan of neuronic arrangement as exemplified in the vertebrates also obtains in animals as lowly organized as the earthworm. In this form the sensory neurones, whose cell-bodies are situated in the integument instead of being gathered into special ganglia, extend, as in the vertebrates, from the skin to the central nervous organs, the brain or the ventral ganglionic chain. The motor neurones are essentially duplicates of those in the vertebrates in that their cell-bodies lie within the central organs whence their fibers extend to the appropriate musculature. Association neurones are also abundantly present in the earthworm though their function here, in contrast with that in the higher vertebrates, is pure nervous intercommunication, for it is very unlikely that the earthworm possesses what in any strict sense of the word can be called intelligence. Thus from a morphological standpoint, the nervous systems of the higher animals, even including such forms as the earthworm, have much in common, their three sets of interrelated neurones, sensory, motor, and association, being arranged upon an essentially uniform plan.

Considered from a physiological standpoint, the nervous system with its appended parts as just sketched falls in the higher animals into three well marked categories. On the exterior of these animals, are to be found sense organs or receptors such as the free-nerve terminations of the sensory neurones in the vertebrates or the sen-

sory cells in the integument of the earthworm. These organs have for their function the reception of the external stimuli and the production of the sensory impulses. The receptors are connected by nerve-fibers with the central nervous organ or adjustor composed of the central ends of the sensory and the motor neurones and of the association neurones. Here the impulses arriving from the receptors are directed toward the appropriate groups of muscles by which the animal may respond to the stimulus and, if the animal is highly organized, impressions are made upon the adjustor which, as memories, may become more or less permanent parts of the animal's nervous equipment. Finally the adjustors are connected by nerve-fibers with the third set of elements, the effectors, which as muscles, electric organs, glands, etc., enable the animal to react on the environment. Thus three physiological categories are to be distinguished which in the order of their sequence in action are sense organs or receptors, central nervous organs or adjustors, and muscle, etc., or effectors.

It is to be noted in passing, that the physiological scheme just outlined includes a wider range of parts than is generally admitted under the head of the nervous system. The additional parts are the effectors, which, as will be shown later, form as truly a part of the whole system as do the sense organs or the central nervous organs. Since the term nervous system does not ordinarily include the effectors, it is perhaps best to designate the whole chain of related parts, receptors, adjustors, and effectors, as the neuromuscular mechanism and in dealing with the origin of the nervous system, it will be found important to keep this relation in mind, for in such an inquiry, the real question that must be confronted is the origin of the neuromuscular mechanism rather than that of the nervous system alone.

The type of neuromuscular mechanism described in the preceding paragraphs in which a group of receptors is connected with a well centralised adjustor which in turn controls a complex system of effectors, is found only in the more differentiated metazoans. Certainly in the simple metazoans, like the jellyfishes, corals, sea-anemones, etc., only the slightest evidence of this type of nervous organization can be discovered. Nevertheless these animals possess a neuromuscular mechanism but on so simple a plan that investigators

have long been inclined to regard it as representing the first step in the differentiation of the neuromuscular organs. This plan of structure is well represented in the sea-anemones. Each of the two chief layers of cells that make up the living substance of the sea-anemone's body consists of three sublayers: a superficial or epithelial layer, a middle or nervous layer, and a deep or muscular layer. The epithelial layer contains, besides many other kinds of cells, large numbers of sensory cells which terminate peripherally in bristle-like receptive ends and centrally in fine nervous branches. These fine branches constitute collectively the middle or nervous layer in which occasionally large branching cells, the so-called ganglionic cells, occur. Immediately under the nervous layer is the deep layer of elongated muscle-cells. The condition thus briefly described is present over the whole of the sea-anemone's body and though the nervous layer is somewhat emphasized in the neighborhood of the mouth, it cannot be said to be really centralised in any part. Hence this type of nervous system has been designated as diffuse in contrast with the centralised type found in the higher metazoans.

Not only is the structure of the nervous system of the sea-anemone appropriately described as diffuse, but in its action this system shows those peculiarities that would be expected from the possession of so diffuse an organization. Since each part of the animal contains its own nerve and muscle, it is not surprising that after isolation many of these parts will respond to stimuli much as they did when they were a constituent of the whole organism. Tentacles, for instance, when freshly cut from the body of a sea-anemone will respond to pieces of food by encircling them, etc., in much the same way as when these organs were parts of a normal animal. Much evidence of this kind has shown conclusively that the nervous system of cœlenterates is no more centralized physiologically than it is anatomically, but is in all respects essentially diffuse.

What is really present in the neuromuscular portion of the sea-anemone's body is a large number of peripheral sensory cells whose deep branching ends connect more or less directly with the muscles, *i. e.*, without the intervention of a true central organ. This neuromuscular system, if described in the terms already used, could be said to be composed of receptors and effectors without an adjustor

or at least with this member present in only a most primitive state. In my opinion this is the condition in most cœlenterates. Judging from the more recent work on the nervous organs of these animals, centralization can scarcely be said to be present at all in hydra; it is but little more pronounced in the sea-anemone; and, though most marked in the jellyfishes, it does not rise even here to a grade that entitles it to comparison with what is seen in such forms as the earthworm. The cœlenterates, then, are animals possessing receptors and effectors but without developed adjustors. Hence the adjustor or central organ is in all probability an acquisition that represents a later stage in the development of the neuromuscular mechanism than that seen in the cœlenterate.

If the cœlenterates represent a stage in the evolution of the neuromuscular mechanism in which sensory cells and muscles are the only important parts present, it is natural to ask if there is not a still more primitive state from which the cœlenterate condition has arisen. On this question several hypotheses have already been advanced. Claus and, subsequently, Chun maintained that originally the nervous system and the muscles were differentiated independently and that they became associated only secondarily. This view has deservedly received very little attention, for not only is it difficult to conceive that an animal would develop receptive ability without at the same time acquiring the power to react, but not a single example among the lower animals is known in which developed nerve and muscle are present and independent of each other.

Much more worthy of consideration than the hypothesis of the independent origin of nerve and muscle is Kleinenberg's theory of the neuromuscular cell. In 1872 Kleinenberg announced the discovery in the fresh-water hydra of what he designated as neuromuscular cells. The peripheral ends of these cells were situated on the exposed surface of the epithelium, of which they were a part and were believed to act as nervous receptors; the deep ends were drawn out into muscular processes and served as effectors to which transmission was supposed to be accomplished through the bodies of the cells. Each such cell was regarded as a complete and independent neuromuscular mechanism, and the movements of an animal provided with these cells was believed to depend upon the simul-

taneous stimulation of many such elements. It was Kleinenberg's opinion that these neuromuscular cells (Fig. 1, *B*) divided (*C*) and thus gave rise to the nerve-cells and muscle-cells (*D*) of the higher animals. In fact he declared that the nervous and muscular systems of these animals were thus to be traced back to the single type of cell, the neuromuscular cell, which morphologically and physiolog-

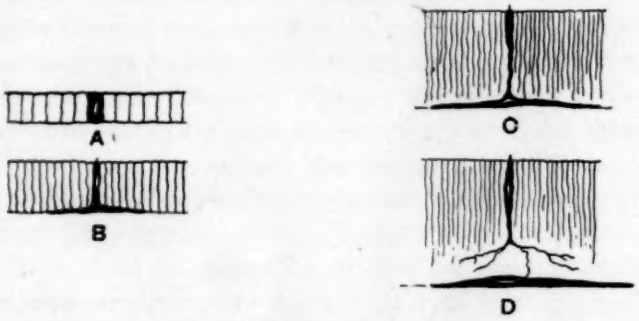

FIG. 1. Diagram to illustrate Kleinenberg's theory of the neuromuscular cell. *A*, epithelial stage; *B*, neuromuscular cell; *C*, neuromuscular cell partially divided; *D*, nerve-cell and muscle-cell of cœlenterate stage.

ically represented the beginnings of both. But Kleinenberg's neuromuscular cells were subsequently shown by the Hertwigs to be merely epitheliomuscular cells and no intermediate stage between them and the differentiated neuromuscular mechanism of higher forms was ever discovered. Hence this hypothesis, too, has been largely abandoned.

Some years later, in 1878, the Hertwigs published an account of the neuromuscular mechanism in cœlenterates, an account which even at the present time is accepted as authoritative by most students of the subject. In this account they described the sensory cells, the ganglionic cells, and the muscular cells of the cœlenterates, and maintained that these elements arose not by the division of single cells, as implied in Kleinenberg's hypothesis, but that each element was differentiated from a separate epithelial cell (Fig. 2) and yet in such a way that during differentiation all these elements were physiologically interdependent. This hypothesis of the simultaneous differentiation of nerve and muscle is the current opinion among biologists today.

As opposed to Hertwigs' hypothesis of the origin of nerve and muscle, I wish to present certain facts obtained from a study of sponges. As is well known, sponges are extremely primitive metazoans, more primitive even than the cœlenterates. All attempts to demonstrate in them sensory or other nervous structures have yielded negative results, so that the majority of investigators of this group have come to regard sponges as devoid of true nervous structures. Not only are sponges without parts that can be reasonably called nervous, but so far as I have been able to ascertain by an extended study of a species of *Stylotella,* they show none of those qualities of transmission and relatively quick reaction which characterize even

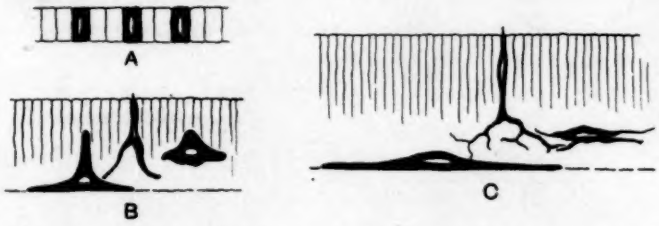

FIG. 2. Diagram to illustrate Hertwigs' theory of the origin of nerve and muscle. *A,* epithelial stage; *B,* partially differentiated muscle-, nerve- and ganglion-cell; *C,* muscle-, nerve- and ganglion-cell of cœlenterate stage.

such animals as have only primitive nervous systems. In fact in this respect sponges resemble plants rather than animals. But notwithstanding the fact that sponges show no evidence either anatomical or physiological of possessing nervous organs, they are not without powers of response. *Stylotella,* for instance, can open and close its oscula and its lateral pores, and can even contract its flesh more or less. These movements, to be sure, are carried out very slowly, but they follow certain stimuli with such regularity that they must be regarded as true responses. Thus the oscula of this sponge close regularly when the water about these openings becomes quiet and reopen after the water has been again set in motion. These movements of the sponge are carried out by contractile tissue which has the appearance of smooth muscle-fiber and which, like smooth muscle, responds with great slowness. The slowness of the response is so marked even in comparison with what is met with among the

more sluggish cœlenterates, as to suggest that the muscles in question act not through the intervention of nerves, but under direct stimulation, and since sponges have yielded no evidence anatomical or physiological of possessing nervous elements of any kind, I have concluded that their muscles normally act under direct stimulation. In other words, sponges are metazoans with effectors but without receptors; and in so far as their neuromuscular mechanism is concerned, they are metazoans one degree simpler than the majority of cœlenterates.

If this conclusion concerning the neuromuscular mechanism in sponges is correct, it follows that, of the three elements concerned, the effector or muscle is the most primitive and has developed as an organ quite independent of nerve, as seen in the sponges (Fig. 3,

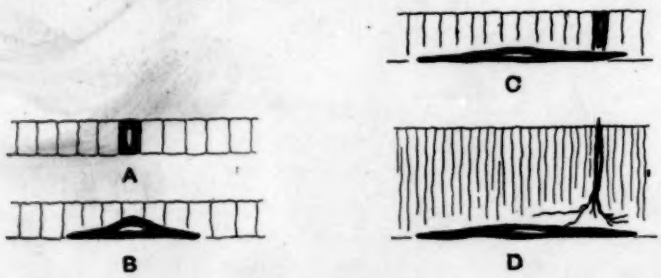

FIG. 3. Diagram to illustrate the early stages in the differentiation of the neuromuscular mechanism. *A*, epithelial stage; *B*, differentiated muscle-cell at stage of sponge; *C*, partially differentiated nerve-cell in proximity to fully differentiated muscle-cell; *D*, nerve- and muscle-cell of cœlenterate stage.

A, B). Next in sequence would appear the receptor or sense organ which, derived from the cells in the neighborhood of a developed effector (*C*), would serve as a more efficient means (*D*) of calling this organ into action than direct stimulation. This stage is represented by many cœlenterates; and their quick responses, as compared with those of sponges, are dependent, I believe, upon this advance in organization. Finally, in forms somewhat more advanced than the cœlenterates, central nervous organs or adjustors would begin to differentiate in the region between the receptors and effectors; and these would develop in the higher animals, first, as organs of transmission whereby the whole musculature of a given form could

be brought into coördinated action from a single point on its surface and, secondly, as the storehouse for the nervous experience of the individual and the seat of those remarkable activities that we recognize in the conscious states of the higher animals. Thus nerve and muscle did not develop independently, as claimed by Claus and Chun, or simultaneously, as maintained by Kleinenberg and the Hertwigs, but muscle appeared first as independent effectors and nerve developed secondarily in conjunction with such muscles, first as a means of quickly setting them in action and, secondly, as a seat of intelligence.

When we survey the whole range of metazoan development, we cannot but be struck with the remarkable history of the neuromuscular mechanism. The earliest metazoans were doubtless little more than colonies of protozoan cells concerned with the common functions of feeding and reproduction and conforming more or less to Haeckel's hypothetical gastræa. To these early functions of this primitive metazoan were added colonial reactions in that a system of independent effectors, more or less such as we see in the musculature of the modern sponge, was differentiated. As a means of bringing this musculature into more effective response, nervous elements developed in close proximity to the effectors. With the growth of the musculature and the nervous system in volume and with the consequent increase of metabolism, came the development of the circulatory system and its dependencies, the respiratory and the excretory organs. Thus the relatively simple body of the primitive metazoan became gradually converted into that of the more complex type. In all these changes no system of organs has done so much to unify the metozoan body as the nervous system. If a sponge may be outlined as a metazoan whose organization concentrates on feeding and reproduction, a human being may be described as one whose organization centers around nervous action. In such an organism the nervous system is supreme; and the rest of the body may be said to do little more than afford a favorable environment for this system; and yet, if the preceding account is correct, this most important system originated somewhat late in the history of the metazoa and as a relatively insignificant organ for the discharge of muscular activity.

HARVARD UNIVERSITY,
April 20, 1911.

THE STIMULATION OF ADRENAL SECRETION BY EMOTIONAL EXCITEMENT.

By W. B. CANNON, M.D.

From the Laboratory of Physiology in the Harvard Medical School.

(*Read April 22, 1911.*)

Dreyer's demonstration that splanchnic stimulation increases the content of adrenal secretion in blood from the adrenal veins has been confirmed by several observers. Adrenal secretion therefore is under control of the sympathetic system.

Major emotional disturbances indicate the dominance of sympathetic impulses. In the cat, for example, fright causes dilatation of the pupils, inhibition of the stomach and intestines, rapid heart, and erection of the hairs of the back and tail. Do not the adrenal glands share in this widespread subjugation of the viscera to sympathetic control?

To try this suggestion the inhibition of contraction in strips of longitudinal intestinal muscle, sensitive to suprarenin 1 to 20,000,000, was used as a biologic test. From the cat when quiet, and again from the animal when excited by a barking dog, blood was obtained by introducing, through the femoral vein, into the inferior vena cava to the region of the liver, a small vaselined catheter. The blood thus obtained was defibrinated and applied to the intestinal strip at body temperature.

After an initial shortening the strip contracted rhythmically in blood from a quiet animal. In no instance did such blood produce inhibition. On the other hand, blood taken from animals after the emotional disturbance, showed more or less promptly the typical relaxing effect. As the emotional period was prolonged, the effect became prompter and more profound.

The view that inhibition of the contracting intestinal strip is due to an increased content of adrenal secretion is justified for the fol-

lowing reasons: (1) The effect was obtained in blood from the vena cava near the liver when that from the femoral vein taken simultaneously produced no inhibition. (2) Removal of the adrenal glands after tying the adrenal vessels resulted in a failure of excitement to produce the effect. (3) Adding varying amounts of adrenalin to inactive blood evoked all the degrees of relaxation that have been observed in excited blood. (4) Excited blood which produced prompt inhibition lost that power on standing or on being agitated by bubbling oxygen. These conditions, together with the evidence that sympathetic impulses increase the secretion of the adrenal glands, and that during such emotional excitement as was here employed signs of sympathetic discharges were observable in the animal from the eye to the tip of the tail, prove that the relaxing effect was due to adrenal secretion.

Injected adrenalin is capable of inducing an atheromatous condition of the arterial wall in rabbits, especially in elderly individuals, and is also capable of evoking hyperglycemia with glycosuria. As Ascher has shown by prolonged stimulation of the splanchnic nerves prolonged adrenal secretion with maintained high blood-pressure can be produced. In the light of the results here reported the temptation is strong to suggest that some phases of these pathologic states are associated with the strenuous and exciting character of modern life acting through the adrenal glands. Two of my students, Shohl and Wright, have recently shown that excitement of the cat results, in all cases thus far examined (more than a dozen), in glycosuria. Possibly in the wild state emotions were useful in providing material for excessive muscular exertion that might follow, and that muscular activity would utilize the sugar so that it would not appear in the urine. This suggestion, however, must be put to further test.

PROC. AMER. PHIL. SOC., L, 199 O, PRINTED JUNE 28, 1911.

THE CYCLIC CHANGES IN THE MAMMALIAN OVARY.

By LEO LOEB.

(*Read April 22, 1911.*)

The observations which I wish to report to you are of interest from several points of view:

1. The process upon which the sexual cycle in mammals depend has been analyzed, and a regulatory mechanism was found to exist within the ovary.

2. A striking illustration is presented of the fact that the structure of organs is in many instances at least not a definite one, but varies in correspondence with the functional condition of the organ.

3. The accurate description of the normal cyclic changes in the mammalian ovary serves as a basis for the investigation of the pathological deviations which interfere with the natural course of the sexual functions and may lead to a temporary or lasting sterility.

4. We found in the ovary structures which must in all probability be interpreted as early stages of embryos developing spontaneously parthenogenetically within the ovary and it is probable that the development of these parthenogenetic embryos is related to certain phases of the sexual cycle.

In the ovary of the guinea-pig definite and very interesting cyclic changes exist which I made the object of my studies in the last few years.

The mammalian ovary consists of two principal constituent parts, namely: First, large and small bodies lined by granulosa cells and filled with fluid, the so-called follicles; and, secondly, the corpora lutea. Both follicles and corpora lutea have only a brief existence; they develop to a certain point and then they degenerate and gradually disappear. The follicles contain the ova. At certain periods of the sexual cycle a few follicles that have reached maturity rupture. The ova reach the Fallopian tubes and uterus and after fertilization

by a spermatozoön form an embryo in the uterine wall. New follicles situated in the periphery of the ovary grow constantly to a certain size and then degeneration sets in. The lining granulosa cells disintegrate and connective tissue grows into the cavity of the follicle. The ova in these degenerating follicles undergo frequently maturation and a few more or less regular divisions and then die. While thus the majority of follicles degenerate, become atretic, before they have reached maturity, a few follicles undergo certain progressive changes and become mature. They may rupture and discharge the ovum; such a rupture is called an ovulation. The process connected with ovulation causes a degeneration of all with exception of the smallest follicles. These follicles grow and in the course of the next six days they have reached that size at which degeneration may set in. We find, therefore, degenerating follicles from the seventh day after ovulation up to the time of the following ovulation. While after the seventh day medium-sized follicles constantly degenerate, new follicles grow and take the place of the degenerating disappearing ones. It seems that it takes approximately ten days until some of the follicles reach their full size.

We may therefore distinguish two periods in the ovarian cycle: First, the period of growth extending over the first seven days following ovulation, and, secondly, the period of equilibrium in which new follicles take the place of degenerating ones. After the first large follicles have appeared, it takes a few days longer until large follicles become transformed into mature follicles that are ready to rupture. We find, therefore, the first mature follicles to appear approximately eleven to thirteen days after ovulation and it would be natural to expect that about fourteen days after the preceding a new ovulation should take place.

The sexual period—that is the period between two ovulations—should therefore have a natural duration of approximately two weeks in the guinea-pig. This is, however, not the case. The sexual period in this species actually lasts about twenty to twenty-five days. And this is due to the fact that a mechanism exists within the ovary that prolongs the sexual cycle. In order to understand this mechanism, we must follow the fate of the ruptured follicle. A follicle

that has ruptured at the time of ovulation does not degenerate in a similar manner as the other follicles do after they have reached full size, but they grow in a remarkable manner and form a new gland-like organ, the corpora lutea. Now these corpora lutea also degenerate after a period of growth that lasts approximately seventeen to twenty days. In the corpora lutea resides the mechanism that prevents a new ovulation. It is necessary that they degenerate before a new rupture of follicles can take place. As long as they function they prevent ovulation. The fact that the corpora lutea degenerate when seventeen to twenty days old, explains why a new ovulation takes place approximately every three weeks. If we excise the corpora lutea at an early date after ovulation, a new ovulation occurs very soon after mature follicles have made their appearance, approximately thirteen to fifteen days after the preceding ovulation. Under these conditions, the normal sexual cycle is reëstablished.

During pregnancy the life of the corpus luteum is prolonged in consequence of the changes occurring in the uterus or developing embryo during the period of gestation and in consequence of the prolonged life of the corpus luteum, a new ovulation is prevented during the whole course of pregnancy. Toward the latter part of pregnancy, the corpora lutea again degenerate and directly after completed labor a new ovulation can take place.

Ovulation, therefore, depends upon three factors: First, upon the maturation of ovarian follicles; secondly, upon the time of degeneration of the corpora lutea; and, thirdly, upon less important, more or less accidental conditions, as for instance, the process of copulation. The third class of conditions accelerates in many (not in all) cases ovulation, but it is not necessary for its occurrence. Even without a preceding copulation, ovulation usually takes place, but in many cases at a later date. Through what mechanism does the life of the corpus luteum influence ovulation? It might be conceivable that the corpus luteum delays the maturation of follicles thus preventing a rupture. My observations have, however, shown that an inhibiting influence of the corpus luteum upon the maturation of follicles does not exist. Mature follicles appear frequently during the life of the corpus luteum, and especially during the period of pregnancy; it

seems that pregnancy even favors the maturation of follicles. The corpus luteum prevents, however, the *rupture* of the mature follicles. Pregnancy as such does not prevent the rupture provided the corpus luteum has been previously removed through excision.

The structural changes in the ovaries are rhythmical and so regular that a careful histological examination of these organs enables us to decide within a certain limit of accuracy at what period of the sexual cycle the animal had been at the time of the removal of the ovaries.

Having established the normal cycle I turned more recently my attention to its pathological deviations. It occurs in a certain number of animals—and I have observed this to happen among females which showed no desire for copulation or in which notwithstanding an accomplished copulation an ovulation did not follow—that the follicles do not grow to maturity, but that they undergo involution before they reach their full size, and that all, or almost all the follicles, become sclerosed, atretic, at a very early stage of their development. Under these conditions, an ovulation is impossible and the animal, in the ovaries of which such a deviation from the normal cyclic changes exists, are at least temporarily sterile; whether such a pathological condition may ever lead to a permanent sterility, future investigations must show. It will be readily understood that here we have to deal with questions of the greatest importance to the physiology of the sexual functions.

In order to appreciate thoroughly the conditions under which such abnormalities in the sexual cycle occur, it is necessary to produce the subnormal development of the follicles experimentally. Now it is of interest to know that such a premature involution of the ovarian follicles can be produced experimentally by burning a certain relatively small part of the ovaries with the thermocautery. The remaining larger part of the ovary remains apparently perfectly well, the cells functionate but the energy of growth of certain cells is diminished and subinvolution of the follicles with resulting temporary sterility follows. A comparable condition can be produced in tumors through heating, or through the influence of certain chemicals exerted in vitro as I found a number of years ago. Under such conditions

the tumors grow, but with markedly diminished energy. In both cases, in the case of the tumors as well as of the ovaries, we have to deal with a state of living matter intermediate between its full natural vigor and latent life; we may regard it a state of partial shock of cells, in which the growth takes place but with a considerable decrease in energy. Besides the changes which I have just described, additional processes of the greatest interest take place in the ovaries of a certain number of animals and it is very probable that these processes usually commence at the period following ovulation and are therefore, in a certain sense, a part of the cyclic ovarian changes. The process I refer to concerns an apparently spontaneous partial parthenogenetic development of ova in the mammalian ovary, an occurrence of which I obtained convincing proof only within the last few months.

Some years ago I described peculiar structures that are found in the ovaries of guinea-pigs, and I expressed the opinion that they originated in the ovarian follicles.[1] Very soon after I had published my observations, certain considerations suggested to me that these structures owe their origin to parthenogenetically developing ova.

In as much as at that time I had not yet seen early stages of the structures referred to, I was unable to regard this hypothesis as sufficiently founded to warrant publication. I continued, however, my investigations in this direction, and recently I succeeded in finding in two animals the desired early stages. They must be interpreted as embryos developing parthenogenetically within the ovary of the guinea-pig. We see in each case a chorionic vesicle with trophoblast, and plasmodia and syncytia penetrating into the neighboring tissues. There is present also a structure which is probably to be interpreted as a neural tube.

Aberrant blastomeres (remnants of dividing ova that failed to participate in the embryonic development) cannot be seen in the ovaries of guinea-pigs, and, inasmuch as the embryonic structures, described in my former communication, are relatively frequent, occurring in approximately ten per cent. of all guinea-pigs below the age of six months, and, furthermore, inasmuch as they are situated

[1] *Archiv f. mikrosk. Anatomie,* Bd. 65, 1905.

in the cortex of the ovaries at a place where follicles lie normally and are found within follicle-like cavities, they can only be derived from ova developing parthenogenetically. Fertilization through spermatozoa can be excluded, inasmuch as the history of some of these animals is known to us and precludes such an interpretation. It is very probable that the parthenogenetic development sets in soon after ovulation, the altered conditions in the ovaries at that time (variations in blood pressure, in intrafollicular pressure or changes in gas exchange) supplying the necessary stimulus. This interpretation agrees well with my former observations concerning the parallelism existing between the first segmentations taking place in non-fertilized ova within the ovary and certain stages of atresia of follicles.[2]

It is also of interest to note that frequently these changes are multiple, several ova undergoing parthenogenetic development in the same ovary.

We may, therefore, conclude that in at least ten per cent. of all guinea-pigs parthenogenetic development of the ova within the ovary starts at some period of the life of the animal. The later stages of these developing embryos bear some resemblance to chorionepitheliomata, certain tumor-like formations consisting of proliferating chorion tissue. During ovulation these structures are occasionally injured by hemorrhages and they are ultimately invaded and supplanted by the neighboring connective tissues.

These observations throw furthermore light on certain interesting tumors that are especially found in the ovaries and testicles, namely: the teratoid tumors and the chorionepitheliomata. My observations are a strong argument in favor of the view that teratoid tumors that are found in the ovaries are not derived from misplaced blastomeres, as Bonnet and Marchand believed, but that the older view is correct according to which they are derived from parthenogenetically developed ova, an opinion which I, also, expressed on previous occasions. The same statement can be made in the case of the chorionepitheliomata that occur in the ovaries and in the testicles. I believe that the observations here recorded clear up the mechanism of the sexual

[2] "On Progressive Changes in the Ova in Mammalian Ovaries," *Journal of Medical Research*, Vol. 1, 1901.

cycle in its essential aspects and they also make it extremely probable that in a relative large proportion of mammalian animals a spontaneous parthenogenetic development of ova takes place at some period during the life of the animal.

Department of Pathology,
Barnard Free Skin and Cancer Hospital,
St. Louis, Missouri.

THE SOLAR CONSTANT OF RADIATION.[1]

By C. G. ABBOT.

(*Read April 21, 1911.*)

If we had no eyes we should still know of the sun by the feeling of warmth. The intensity of solar rays in any part of the spectrum can be measured by delicate thermometry. Vision and photography are both restricted within comparatively narrow limits of wave-length, and each differs in its sensitiveness from wave-length to wave-length. Ultra-violet, visible and invisible red rays, however, all produce their just and proportional influences on the bolometer, or thermopile. This is not universally known, and there are still many who suppose we should distinguish between so-called actinic, visible and heat rays. Doubt has been expressed, for instance, whether bolometric measurements give true indications of the intensity of those rays which promote plant growth. Such doubts are not justified, and we may expect very valuable results in the future from the application of the spectro-bolometer to the interesting questions of radiation and plant physiology.

We use heat units to express the intensity of solar radiation. The solar constant of radiation may be defined closely enough as the number of degrees by which one gram of water at 15° centigrade would be raised, if there should be used to heat it all the solar radiation which would pass at right angles in one minute through an opening one centimeter square, located in free space, at the earth's mean solar distance. Experiments were begun about 1835 by Pouillet and by Sir John Herschel for the measurement of this great constant of nature. The investigation has been continued by Forbes, Crova, Violle, Radau, Langley, K. Ångström, Chwolson, W. A. Michelson, Rizzo, Hansky, Scheiner and others. It is an indication of the great difficulty of the research that entire uncer-

[1] Published by permission of the Secretary of the Smithsonian Institution.

tainty as to the value of the solar constant of radiation between the limits of Pouillet's value, 1.76 calories, and Ångström's value, 4.0 calories per square centimeter per minute, prevailed at the beginning of the twentieth century.

Professor Pringsheim has collected the following table[2] of solar-constant values, as determined by different observers:

Year.	Observer.	Calories.	Year.	Observer.	Calories.
1837	Pouillet	1.8	1889	Pernter	3.2
1860	Hagen	1.9	1896	Vallot	1.7
1872	Forbes	2.8	1897	Crover and Hansky	3.4
1875	Violle	2.6	1898	Rizzo	2.5
1878	Crova	2.3	1908	Scheiner	2.3
1884	Langley	3.1	1908	Abbot and Fowle	2.1[3]
1889	Sawelief	2.9			

He omits Ångström's 4.0, published in 1890 and withdrawn in 1900, but which is even yet sometimes quoted. He omits also Very's 3.1, published in 1901 and independently obtained in 1910. Recently published values of Kimball, Gorczynski and others, approximately 2.0, are based in part on work of Abbot and Fowle.

The determination of the solar constant involves: (1) correct measurements of the heat equivalent of the solar radiation at the earth's surface; (2) a correct estimate of the losses which the rays have suffered in the atmosphere before they reached the measuring apparatus. We shall now discuss these two branches of the work.

Pouillet invented, about 1835, his well-known instrument, the pyrheliometer, for measuring the solar rays at the earth's surface. Many criticisms have been justly made in regard to the accuracy of this pioneer instrument, and attempts have been made by many to improve on it, or to substitute a better. In our practice at the Smithsonian Astrophysical Observatory, we have substituted a silver disk for Pouillet's water chamber; inserted a cylindrical bulb thermometer, radially instead of axially, in the disk; provided a metal-lined wooden chamber to screen the instrument from the wind; and added convenient adjuncts for shading and exposing the instru-

[2] "Physik der Sonne," p. 417.

[3] This value was expressed in terms of a provisional scale of pyrheliometry which has since been proved too high.

ment.[4] Finally we have ceased to regard our instrument as giving more than relative measurements. It is only a secondary pyrheliometer for convenient use. We standardize its readings by comparison with an absolute pyrheliometer of another kind.

No known substance absorbs radiation perfectly at a single encounter. Kirchhoff showed, fifty years ago, that a hollow chamber must absorb perfectly, because of the opportunity for an infinite number of absorption encounters within it. W. A. Michelson, in 1894, invented a standard pyrheliometer including a hollow chamber with a narrow opening for the admission of rays. The walls of the chamber were bathed by a mixture of ice and water, and the heating effect of the solar rays was measured by the amount of ice melted, which was determined by noting the expansion in volume of the mixture of ice and water.

Nearly ten years later, being ignorant of Michelson's pyrheliometer (which was described in the Russian language), it occurred to me also to employ a hollow receiving chamber. I proposed to measure the solar heating produced in it by bathing its walls with flowing water, and determining the rate of flow and rise of temperature of the water. After experiments lasting intermittently from 1904 to 1910, I am now satisfied that this device has proved successful, and that we have truly an absolute standard pyrheliometer. With the aid of my colleagues, Mr. Aldrich and Mr. Fowle, two of these water-flow pyrheliometers were carefully tested last year.[5] Not only did they agree in measurements of solar radiation, but test quantities of heat introduced electrically within the absorbing chambers were accurately recorded by the methods ordinarily used to measure solar heating. We believe of the absolute water-flow pyrheliometer, it gives the intensity of solar radiation at the earth's surface in calories per square centimeter per minute within a probable error of 0.2 per cent. For convenience we make our daily observations with secondary silver-disk pyrheliometers, which have been standardized against the absolute water-flow pyrheliometer.

[4] See Abbot, "The Silver Disk Pyrheliometer," *Smithson. Misc. Coll.*, Vol. 56, No. 19, 1911.

[5] See Abbot and Aldrich, *Astrophys. Journal*, Vol. XXXIII., 125, 1911.

Having perfected the standard and secondary pyrheliometers to a satisfactory degree of accuracy and durability, the first branch of solar-constant work is accomplished by reading with the silver-disk pyrheliometer at the earth's surface, and reducing its indications to calories per square centimeter per minute. We now turn to the discussion of the second branch of the work, namely the estimation of the transmission of the atmosphere for radiation.

Lambert and Bouguer showed almost simultaneously, about 1760, that the transmission of light through a homogeneous medium may be expressed by an exponential formula, such as:

$$E = E_0 a^m.$$

Here E is the intensity transmitted, E_0 the original intensity, a the fraction transmitted by unit thickness, and m the actual thickness of the transparent medium.

Pouillet applied Bouguer's formula to the atmosphere. As the atmosphere is not homogeneous, but decreases in turbidity and density from the earth's surface upward, this would seem at first sight unjustified. But if we consider unit thickness to be the thickness of the atmosphere traversed by a vertical beam, then as the ray departs from the vertical, it still shines through every layer which it did at first, and the path in every layer increases nearly as the secant of the zenith distance of the ray. Under these circumstances it can be shown that (subject to certain limitations to be mentioned) the exponential formula given above should hold, if we consider E to be the intensity at the earth's surface, E_0 the intensity outside the atmosphere, a the transmission coefficient for a vertical beam, and m the secant of the sun's zenith distance.[6]

Owing to atmospheric refraction, the fractional increase in path of the ray, as the zenith distance waxes, tends to be greater for the outer layers of the atmosphere than for its inner ones. On the other hand, the curvature of the earth's surface produces an opposite tendency. But for zenith distances less than 70° these effects may be neglected, and they are hardly worth considering at 75°

[6] See *Annals Smithson. Astrophys. Obser.*, Vol. II., p. 14, 1908.

zenith distance.[7] Solar-constant determinations require no higher values of zenith distance than these to be considered.

Radau and Langley proved the necessity of confining the atmospheric use of Bouguer's formula to approximately monochromatic rays. In general, for a reason which Lord Rayleigh has shown, the transmission of the atmosphere increases gradually with increasing wave-lengths. Thus in the violet the transmission for a vertical ray to sea level may be 50 per cent., and for a deep red ray 80 per cent. But besides this gradual change there are also spectral regions of almost complete absorption by atmospheric oxygen, and by water-vapor, so that in these regions the transmission approaches zero. If we should disregard these differences, and determine the constants of the exponential formula above, by pyrheliometric measurements alone at different solar zenith distances, our result E_0 for the intensity outside the atmosphere must necessarily be too small.[8]

Langley was the first to act upon this, and to devise apparatus and methods for measuring the energy and the atmospheric transmission at all parts of the spectrum. For this purpose he invented the bolometer about 1880, and automatic registration of its indications about 1890. As we now use it the bolometer comprises two similar tapes of platinum, each about 1 cm. long, 0.01 cm. wide and 0.001 cm. thick. These are coated with lamp-black by smoking over a camphor flame. They lie parallel to the spectrum lines, and about 0.8 cm. apart. One tape may be shined upon by the rays, the other can not. Hence the heat absorbed from a narrow region of spectrum, usually about twice the extent comprised between the D lines, raises the temperature of the exposed tape with reference to the other. The two tapes and two resistance coils are combined to form a Wheatstone's bridge, and the rise of temperature produced as above stated deflects a sensitive galvanometer. The galvanometer needle reflects a tiny spot of light on a photographic plate, which moves vertically as driven by clock work. The same clock work moves the spectrum slowly over the bolometer tape. In this way may be produced in from eight to twelve minutes, according to the spectroscopic outfit employed, a bolograph, or spectrum energy

[7] *Loc. cit.*, p. 59.
[8] *Loc. cit.*, p. 16.

curve, extending from about wave-length 0.30 μ in the ultra-violet to about wave-length 3.0μ in the infra-red. Its ordinates are deflections of the galvanometer, proportional to energy in the spectrum, and its abscissæ are proportional to differences of prismatic deviation. The Fraunhofer lines, and great oxygen and water-vapor bands, show as depressions in the curve. In order to eliminate distortions which are due to differences, for differing wave-lengths, in the reflecting power and transmission of the mirrors and prism used in the optical train, special investigations of the transmission of the apparatus are made from time to time, and the curves corrected accordingly.

In our ordinary practice, from six to eight bolographs are taken in a single forenoon, between the times when the sun's zenith distance is 75° and (say) 30°. The curves are measured at about thirty positions, uniformly spaced in the prismatic spectrum. Each group of six to eight measurements, at a single spectrum place, furnishes means of computing from Bouguer's formula the transmission of the atmosphere for that wave-length, and also the ordinate which would have been found there if the observations had been made outside the atmosphere. The sum of the ordinates measured on any bolograph is approximately proportional to the total energy of all wave-lengths observed. Similarly the sum of the ordinates computed for outside the atmosphere is proportional to the total energy there.[9]

In order to reduce the total energy, as determined bolometrically, to calories per square centimeter per minute, the pyrheliometer is read, while the spectro-bolometric work is in progress, on each day of observation. Thus a factor is obtained for deducing from the areas of the bolometric curves the true heat units corresponding.[10] A complete determination of the solar constant of radiation requires

[9] In the regions of great water-vapor and oxygen absorption the extra-atmospheric curve is determined by interpolation between adjacent comparatively unaffected wave-lengths on either side, for we know that there is no oxygen or water-vapor absorption of these bands produced in the sun, so that they ought not to show in the extra-atmospheric curve. Small allowances are also made for the energy of lesser and greater wave-lengths than any observed.

[10] For further details consult *Annals*, Vol. II.

about three hours of observation, under a cloudless and uniformly clear sky, and about three days of computing.

We began to make solar-constant observations in Washington at the Smithsonian Astrophysical Observatory, in October, 1902, and continued them there whenever a favorable opportunity was presented, until May, 1907. In all this time we made only 44 tolerably satisfactory determinations at Washington, for cloudless days were rare, and many days that promised fairly proved disappointing, by reason of the appearance of smoke, haze or clouds. Four important results came from the Washington observations. First, no apparently good determinations yielded solar-constant values above 2.38 of our then provisionally adopted scale of calories, or 2.25 true calories. Second, the mean value in the true calories from 44 determinations was 1.960. Third, the transmission of the atmosphere was determined on many days, and for many wave-lengths.[11] Fourth, a strong probability was raised by the results of observations of 1903 that the sun is a variable star.[12] This variation seemed to reach 10 per cent. in its extreme range, but no tendency towards a regular period was then found for it. A dependent variation in terrestrial temperatures seemed indicated.

Primarily in order to make spectro-bolometric determinations of the solar constant, suitable to test the supposed variability of the sun, an expedition under my charge went out to Mount Wilson in 1905, by invitation of Director Hale of the Mount Wilson Solar Observatory. The site proved excellent for the purpose, on account of its considerable altitude, cloudless sky and freedom from wind. Much aid and comfort was furnished by Director Hale and his staff. The expedition was repeated in 1906, 1908, 1909 and 1910. We now occupy a cement observing shelter and living quarters there, on ground leased from the Solar Observatory. Our observations have generally occupied the six months, May 15 to November 15, and in the last years we have made practically daily determinations of the solar constant of radiation during this interval.

[11] Astronomers have not yet very generally availed themselves of the accurate coefficients of atmospheric transmission obtained in our researches for all parts of the spectrum, and from Washington, Mount Whitney and Mount Wilson.

[12] See S. P. Langley, *Astrophysical Journal*, Vol. 19, p. 305, 1904.

It was thought doubtful by Langley, and others, if correct estimates of the atmospheric transmission can be made, even by the spectro-bolometric method of high and low sun observations. Langley, indeed, gave an argument tending to show that the values of the solar constant thus obtained fall far below the true intensity of the solar radiation outside the atmosphere. This argument, however, seems to be unsound.[13] In order to test the accuracy of the method I made spectro-bolometric measurements on Mount Whitney (4420 meters elevation) in 1909 and 1910 simultaneously with similar observations made by Messrs. Ingersoll and Fowle, respectively, on Mount Wilson (1800 meters elevation). In 1905 and 1906 solar-constant measurements were made nearly simultaneously at Mount Wilson and at Washington (10 meters elevation). It does not appear from these observations that there are any differences in the solar-constant values depending on the altitude of the observer, and not due to accidental errors of observations.[14]

In illustration of this conclusion I give the results obtained simultaneously at Mount Wilson and Mount Whitney:

Date.	1909, Sept. 3.	1910, Aug. 12.	1910, Aug. 13.	1910, Aug. 14.
Mount Wilson.....	1.943	1.943	1.924	1.904
Mount Whitney.....	1.959	1.979	1.933	1.956

The very slight excess of the Mt. Whitney values is not large enough to be significant.

We conclude that the solar-constant values computed from the method of high and low sun observations do not depend on the altitude of the observing station up to altitudes of 4,420 meters, provided the sky conditions are satisfactorily clear and uniform.

Reducing values published in Vol. II. of the *Annals* to standard calories at 15° centigrade, and including the mean values obtained in later years,[15] we have:

[13] See *Annals*, Vol. II., pp. 119–121.

[14] As regards the Washington and Mount Wilson comparisons, see *Annals*, Vol. II., pp. 99 and 102. Note that the provisional scale of these *Annals* values is 5 per cent. too high.

[15] Many of the values of 1910 are not yet reduced.

SOLAR-CONSTANT MEAN VALUES.

Place.	Washington.	Mount Wilson.					Mount Whitney.	
Date.	1902–1907	1905	1906	1908	1909	1910	1909	1910
Times observed.....	44	59	62	113	95	28	1	3
Mean value.....	1.960	1.925	1.921	1.929	1.896	1.914	1.959	1.956

Our observations indicate as the mean value of the solar constant of radiation:

1.922 calories (15° C.) per square centimeter per minute.

The observations having been obtained mainly near the time of sun-spot maximum we think it probable that their mean is hardly high enough to represent the average condition of the sun. We incline to think this because it has been shown by Koppen, Nordmann, Newcomb, Abbot and Fowle, Bigelow, Arctowski and others that the earth's temperature is a little lower at sun-spot maximum than at sun-spot minimum. This probable correction cannot exceed one or two per cent.

There is another reason why our value of the solar constant may be too low. We have not been able to observe, even on Mount Whitney, any radiation beyond the wave-length 0.29μ in the ultra-violet spectrum. Whether the rays of less wave-length are obliterated in the earth's atmosphere or in that of the sun we cannot know, but we do know that ozone, which is perhaps formed in the upper atmosphere, exercises powerful selective absorption beyond wave-length 0.29μ. Hence it may be that we are forced to neglect some radiation not quite negligible. It is very improbable that the amount thus neglected exceeds 1 or 2 per cent.

As for the supposed variability of the sun, our determinations strongly indicate that the so-called solar constant is not really a constant, but fluctuates over a range of about 8 per cent. This result is apparently the direct outcome of our observations, but the question may well be asked if the apparent fluctuation is not due either to the inaccuracy of the observations or to incorrect estimates of the transmission of the atmosphere. If it were due merely to accidental errors of observations, a gradual march, step by step, day by day, from a low value to a high one and return would be the ex-

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ception. We find it to be the rule, hence we must exclude accidental errors as the main source of the apparent variability of the sun. As for the other explanation suggested, we find no material difference in the result derived for the solar constant on a good day whether we observe at sea-level, at one mile, or at nearly three miles elevation, though the pyrheliometer readings on the ground differ by 25 per cent. between Washington and Mount Whitney. Hence we may reasonably conclude that we do, in fact, correctly estimate the loss which occurs in the atmosphere. The fluctuation in the solar-constant results therefore seems to indicate either a true variability of the sun, or else the interposition of meteoric dust, or other cosmic hindrance to the passage of radiation from the sun to the earth. These fluctuations, while not of regular periodicity, generally run their courses within five or ten days.[16]

It is now proposed to test this conclusion by conducting solar-constant measurements simultaneously at Mount Wilson and in southern Mexico. If the results of a long series of daily observations at these remote stations should agree, it would seem quite unlikely that any apparently simultaneous fluctuations of the solar constant of radiation could be attributed to terrestrial influences.

Summary.

Special apparatus, including the silver-disk secondary pyrheliometer, the absolute water-flow pyrheliometer and the recording spectro-bolometer, has been employed by the writer and his colleagues at Washington and Mount Wilson and Mount Whitney, to determine the mean value of the solar constant of radiation, and its possible fluctuations.

The observations, exceeding 400 in number, have been made in all the years since 1902 to 1910, but most plentifully and accurately in 1908, 1909 and 1910. The mean value of the intensity of solar radiation outside the atmosphere, at mean solar distance, is found to be 1.922 (15°C.) calories per square centimeter per minute, but might prove 1 or 2 per cent. higher in years of less sun-spot activity. The solar-constant values do not appear to depend on the altitude of

[16] See Abbot and Fowle, *Astrophysical Journal*, April, 1911.

the observing station, up to the highest altitude tested, 4,420 meters. Fluctuations in the values proceeding step by step, day by day, from higher to lower values and return, within a range of about 8 per cent. usually occur in somewhat irregular intervals of from five to ten days in total period. These fluctuations are thought to indicate a true variability of the sun. It is proposed to test this conclusion by daily observations extending over several months, and to be made simultaneously in California and southern Mexico.

SELF-LUMINOUS NIGHT HAZE.

By E. E. BARNARD.

(*Read April 21, 1911.*)

There is one phase of the night skies which does not seem to have received much or any attention. It is the occasional presence of self-luminous haze. This matter does not seem to be similar to the luminous night clouds, "die leuchtenden Nachtwolken," which were observed by O. Jesse and others some twenty-five or thirty years ago, and which were found to be clouds at such great altitudes above the earth's surface (upwards of fifty miles high) that they received the sunlight long after or before the ordinary clouds. The observations of O. Jesse were printed in the *Astronomische Nachrichten,* Bd. 121, pp. 73, 111; Bd. 130, p. 425; Bd. 133, p. 131; Bd. 140, p. 161. In *A. N.*, Bd. 140 (No. 3347), he gives a long list of altitudes, determined by photography, which range from 81 km. to 87 km. The mean value given by the observations from 1885 to 1891 was 82 km. (52 miles). These clouds were seen in the northern hemisphere only near the time of the summer solstice. In the southern hemisphere they were seen at the opposite season. From his papers it is clear that these sunlit clouds were in no way related to the present subject, and I only mention them to forestall any suggestion that they were similar to the ones seen by me. The objects to be described here were apparently at the altitude of the ordinary higher clouds. They have been seen in all parts of the sky and at all hours of the night. In a paper on the aurora[1] I have previously called attention to the frequent luminous condition of the sky at night. This feature long ago impressed itself upon me. Indeed any one who has spent much time under the open sky hunting comets, etc., will have been forcibly impressed with this peculiarity. In most cases this illumination has been due evidently to a diffusion of the

[1] "*Astrophysical Journal,* 31, April, 1910.

general star light, perhaps by moisture in the air. This latter condition is present as a whitening of the sky, which gives it a "milky" appearance. At other times the sky is more or less feebly luminous, but the luminosity is different from the other condition and is evidently not due to a diffusion of star light. In reality the sky seems to be self-luminous. Sometimes the whole sky has this appearance, and at other times a large portion only. At times the illumination is so great that the face of an ordinary watch can be read with no other light than that of the sky. It is indeed seldom that the sky is rich and dark. In any determination of the total amount of the light of the sky the results must be uncertain because of the great changes that so often take place in the amount of the illumination. The self-luminous condition frequently occurs when no ordinary indications of an aurora are present. It is, nevertheless, doubtless of an auroral nature, for Professor Campbell has shown that the spectrum of the aurora is essentially always present on a clear dark night. (*Astrophysical Journal*, 2, August, 1895, p. 162.)

I have given an account[2] of the remarkable pulsating clouds of light that are seen here occasionally and which usually, but not always, have an easterly motion—generally southeast. They are mostly confined to the northern half of the heavens. There is another phenomenon that has been visible on a number of nights of last year, and also in the present year, of which I have seen no record. This consists usually of long strips of diffused luminous haze. I believe that this is really ordinary haze, which for some reason becomes self-luminous. It is not confined to any particular region of the sky nor to any hour of the night. It always has a slow drifting motion among the stars. This motion is comparable with that of the ordinary hazy streaky clouds that are often seen in the daytime. They are usually straight and diffused and as much as 50° or more in length and 3° or 4° or more in width. In some cases they are as bright, or nearly as bright, as the average portions of the Milky Way—that is, they are decidedly noticeable when one's attention is called to them. They apparently are about as transparent as ordinary haze. Sometimes, when seen near the horizon, where

[2] *Astrophysical Journal*, 31, April, 1910, p. 210, etc.

they may be quite broad, they have strongly suggested the "dawn" or glow that precedes a bright moonrise. Their luminosity is uniformly steady.

The reason I speak of this matter as haze, and the reason I think it is only ordinary haze made self-luminous, is because on one occasion I watched a mass of it in the northwestern sky which was slowly drifting northerly in the region of the great "dipper" of Ursa Major as daylight came on. These hazy luminous strips had been visible all the latter part of the night—new strips coming and going slowly, sometimes several being seen at once. As daylight killed them out I noticed, when the light had increased sufficiently, that there were strips of ordinary haze exactly the same in form and motion and occupying the same region of the sky. I am sure they were the same masses that had appeared luminous on the night sky. My impression, therefore, is that these hazy luminous strips were only the ordinary haze which had for some reason become self-luminous. I am specially certain that these masses are not luminous as a result of any great altitude which might bring them within reach of the sun's light, for they were frequently seen in such positions that the sun's rays could never reach them. The sun or moon, therefore, had nothing to do with their illumination. It is also needless to say that they are not related to the pulsating auroral clouds which I have previously mentioned.

I have not noticed this luminous haze in former years, though it may have been present, and did it not seem unreasonable, one might suspect some relation between this condition of the atmosphere and the possible passage of the earth through a portion of the tail of Halley's comet on 1910, May 19.

I will give here the observations which I have obtained of these singular features. It seems to me that these objects should be observed and a record made of the times of their visibility and their motion, etc. It would be valuable to have records of them from different stations to see if their luminosity is due to some general condition of the earth's atmosphere at the time. It is not probable that this luminosity is in any way due to local conditions. In the records here given, it is possible that on one or two occasions an

aurora was also present, but I have tried to confine the accounts to what I have called, and believe to be, self-luminous haze. They were not seen previous to June 7, 1910.

The Observations.

1910, June 7d 13 h 0m. These diffused luminous masses were seen in different parts of the sky. They were specially noticeable near the southern horizon where the appearance was that of a definite whitish light stretching along above the horizon for a considerable distance. Long bands of this matter were parallel with the southern horizon and above Antares. In the east a long strip 3° or 4° wide stretched from α Pegasi to α Andromedæ and beyond. This moved slowly eastward. At 13h 30m another was passing through the bowl of the great "dipper" in the northwest with a slow easterly motion. A very broad one was situated about 15°–20° from the zenith to the west. They were about as bright as the Milky Way in Cygnus. I waited until near sunrise, and could then see a long mass of ordinary haze, reddish with sunlight, occupying the position of one of the strips that was seen near the bowl of the "dipper," which had been visible as a luminous mass until the dawn had killed it out. There were other strips and masses of haze at different points in the sky when the sun rose. I think it was these streaks and patches of diffused haze that were luminous during the night. They appeared as ordinary haze clouds in daylight. During the entire night there had been no ordinary trace of aurora.

June 9. Though they were looked for several times none was seen until about 10h 30m or 11h 0m. At 11h 25m a long broad hazy streak, as bright as the Milky Way in Cassiopeiæ was seen in the northwest. The lower end was in the "sickle" of Leo near the horizon. Its upper end was 15° below the polar star. From a sketch at 11h 25m the following points were taken which were involved in the hazy strip:

α 10h 5m δ + 21°.5,
α 9 .5 δ + 49.

It extended beyond this latter point for quite a distance—roughly to about

α 7h 0m δ + 67°.

The stars were visible through it where it passed over them. The motion was slowly to the northwest among the stars. Its width was 5°. At 12h 0m a similar band passed over the "dipper" parallel to the first one, evidently moving in the same direction. The first one at this time had either disappeared or was too near the horizon to be seen. At midnight I could read the time by my watch with only the illumination from the sky, which was milky and whitish or luminous.

June 10d 10h 45m. A long strip passed through Polaris and 5° below the bowl of the "dipper." Its motion was towards the north by east horizon. 11h 0m a great number of luminous masses were scattered over the western sky (and extending to the south) to nearly as high as the zenith. These were mostly parallel strips with some irregular masses. They extended from the horizon and seemed to diverge upwards.

September 29. The sky was irregularly covered everywhere with a kind of luminous haze which occurred in great areas and in strips, with a few clear spaces between which were relatively dark. They were more or less conspicuous. At 8h 25m a diffused luminous band stretched from Corona Borealis to the southwest horizon —nearly north and south. This continued northerly nearly to the pole and was diffused to the west. In the south and southeast for 20° above Fomalhaut to α Ceti was the upper edge of a luminous mass of haze covering the southeast sky to the horizon. Other diffused areas of this matter were visible at different points over the sky. The whole sky was more or less luminous, but less noticeable than the regions described above. By 8h 50m the broad luminous strip at Corona Borealis had drifted a little east among the stars, but it seemed to go westward with them. At 11h 10m a watch could be read by the light of the sky. This was one of the brightest of the luminous nights that I have seen. The matter seemed to be only ordinary haze but luminous for some reason. There was no trace of aurora. The sky on which the luminous haze was seen was, at this time, brightened with a pale uniform illumination covering the entire heavens and nearly blotting out the Milky Way. These masses had very little motion. The sky was too luminous for long exposures with a portrait lens.

September 30. At 9h 15m for 10° above the east by north horizon a broad luminous band 50° long was seen just above and involving Aldebaran. It stetched to the south of the east point and in brightness resembled the appearance produced by the moon just before it rises. The light was soft, yet conspicuous. At 10h 10m under Capella was a large soft diffused light—diffusing to the east and beyond. This light was steady with no fluctuations. Nothing of a similar nature was visible in the north or elsewhere. The sky was dull and more or less luminous. At 10h 55m the illumination extended half way up to Aldebaran and the sky near the horizon was luminous like moonrise. This extended from 25° south of east to nearly due north, rising much higher under Capella —a very soft and steady illumination. 12h 0m. The illumination was feeble and diffused. At 12h 30m it was very feeble and mostly in the northeast—scarcely noticeable. At this time dark smeary haze was visible all over the south. No evidence of an ordinary aurora was seen during the night. The sky was luminous all over, but not so much so as on the twenty-ninth.

October 1. There was a bright aurora.

October 2, 8h 0m. A pale illumination was seen in the low north and also in the low east. The effect was probably auroral.

October 6. The night was more or less luminous and misty.

October 28. There was a luminous sky at night.

October 30, 13h 0m. The night was very luminous with fully as much light as would be caused by a one quarter full moon. The Milky Way was scarcely visible. Watch easily read by the glow at 14h 0m. At 15h and 15h 30m a luminous haze covered all the low northern sky as high as half way to the pole. This was not strong and did not look like an aurora. It seemed simply to be luminous haze.

November 1, 12h 15m. The sky was remarkably luminous everywhere. In the north from the horizon to halfway to the pole the sky appeared more luminous than elsewhere. No trace of an arch. The illumination did not look like that from an aurora, but at 15h 30m a strong auroral arch had formed.

November 10, 12h 0m. There was a great amount of luminous

haze in the north and northwest. At 15h 0m a large mass 10° high was visible in the northwest. Later there was a long diffused strip, 10° wide, which cut the Milky Way at right angles 20° above α Cygni. It was 40° or 50° in length and did not fluctuate. Its appearance was that of luminous haze. Below it was a region of luminous haze that extended to the north.

1911, February 28, 15h 30m. For 20° to 25° altitude all over the east and northeast the sky was luminous with a soft auroral light. There was no arch or intensification near the usual place for an aurora. This was not due to the presence of the Milky Way at that point.

March 2, 8h 10m. A long mass of luminous haze 6° or 8° broad was visible below β Leonis in the east. It diffused down to the northeast horizon. It seemed to be brighter at times, but there were no certain fluctuations of its light. It was not bright. 8h 50m. The region of luminous haze was passing over Arcturus and moving towards the east horizon. It was nearly horizontal and 30° long with the north end the lowest. 10h 45m. A long mass of luminous haze was visible one half way from Spica to the southeast horizon. It extended south as far as Corvus and inclined to the southeast horizon. It was quite bright and steady in its light. All of the southeastern sky strongly resembled the glow from an expected moonrise. 11h 35m. A strong glow from the southeast horizon extended up to 15° or 20° above Jupiter—like a strong moonrise—all along from the east to the south and diffusing upward. It was conspicuously strong. By this time the sky was increasing in luminosity. In the meantime there had been no trace of aurora during the night. These were the first of the luminous masses of haze that I had seen for a long time, except that of February 28, when it appeared near the northeast horizon.

Since the above observations I have not seen any of this luminous haze on the few clear nights that we have had in the absence of the moon.

Yerkes Observatory,
April 4, 1911.

Note.

Since this paper was in type Mr. C. F. Talman, Librarian of the Weather Bureau at Washington, through Dr. W. J. Humphreys, has called my attention to a paper, No. 22, of the Publications of the Astronomical Laboratory at Gröningen, "On the Brightness of the Sky and the Total Amount of Starlight" by L. Yntema. Dr. Yntema calls attention to the frequent luminous condition of the sky and its effect on determinations of the amount of starlight. In section 14 of his paper, which is devoted to earthlight, he gives numerous records of this illumination. There does not appear to be any direct reference, however, to the main features of my paper—the luminous hazy strips and masses.

May 15, 1911.

SPECTROSCOPIC PROOF OF THE REPULSION BY THE SUN OF GASEOUS MOLECULES IN THE TAIL OF HALLEY'S COMET.

By PERCIVAL LOWELL.

(*Read April 21, 1911.*)

1. The return of Halley's comet has been noteworthy chiefly for the possibility of employing upon it modern methods of instrumental research. Since its last previous apparition have been devised those two great engines of astronomic exploration, spectroscopy and celestial photography. The former has afforded us our first direct knowledge of the substances composing comets, while the latter has given us a means of easy and rapid registration of the visitant's appearance. This is especially valuable in the case of a body as vast and vague as a comet, free-hand drawing of which is peculiarly liable to distortion.

During the last return of Halley's comet that body was subjected at Flagstaff to investigation by both instruments simultaneously. One result of this was the detection that gaseous molecules—in contradistinction to minute solid particles merely—are directly repelled by a force emanating from the sun, presumably the pressure of light. Previously this had been held impossible. Schwarzschild had, as he thought, demonstrated mathematically in an able paper that molecules of gas were too small to be thus affected by the forces concerned and Arrhenius had adopted his deduction and published it as a fact in his "Worlds in the Making." That the bodies themselves would so soon refute this would not have been deemed probable and invests the detection with the more immediate interest. Incidentally we may remark that Schwarzschild had since given up his original opinion.

2. That the tail of a comet is due to repellant force exerted by the sun is apparent from the direction the tail takes. For that direc-

tion agrees with what would be shown by particles leaving the nucleus and travelling in hyperbolic orbits away from the sun, the sun being in the full or the empty focus according to the speed of recession.

Although the general fact is thus evident, to measure the recession directly is to obtain both an observational proof of it and also something approaching an exact value of the velocity at a given time and place. Accordingly I determined to do this in the case of Halley's comet at its recent apparition. At my disposal were the two hundred photographs taken of it at the Lowell Observatory between April 18 and June 6. To obtain trustworthy results the photographs to be compared must not be separated by too long an interval, since with time a general commingling of the various particles takes place which not only renders particular decipherment of different outbursts impossible but entirely alters the actual speeds. In the case of Halley's comet this difficulty was enhanced by the unusual uniformity of the tail. Irregularities, bunches or knots were rare; the tail presenting as a rule, a remarkably orderly deportment, dishearteningly same. Among the many plates, however, I was able to select a pair taken seriatim capable of recognition and measurement. Some of these handles to investigation were in the nature of bunches of matter, some of abrupt changes in its direction looking like promontories along the general line of the tail. I chose four of the more salient excrescences and selecting identical features of them in the two negatives measured their respective distances from the nucleus on the two plates. The first plate was exposed from 9h 23m to 9h 53m and the second from 10h 0m to 10h 53m, so that the one followed directly on the other.

When the angular amounts of the changes in place of the several knots were corrected for differential refraction and then reduced to speeds, account being taken of the distance of the comet from the earth and of the inclination to the line of sight of the respective positions along the tail, the results came out as follows:

From these measurements the fact emerges unmistakably that a repulsive force directed away from the sun acted upon the particles on the tail.

TAIL OF HALLEY'S COMET.

	Angular Distance from the Nucleus to the Point Measured in the Tail.	Velocity of the Point of the Tail Away from the Nucleus.
Knot 1	1° 28′	13.6 miles a second
Knot 2	3° 12′	17.2 " " "
Knot 3	4° 36′	19.7 " " "
Knot 4	6° 15′	29.7 " " "

3. While the series of direct photographs was being taken two series of spectrograms were being carried on by Dr. Slipher, one with an objective prism; the other set through a slit. The objective prism ones recorded simultaneously the spectrum of the nucleus and head, together with that of the tail out to about 11° from the nucleus. One of them was got on May 23 at the same time as the photographs measured; while others were obtained on dates before and after. Of the direct information afforded by these spectrograms of the constitution of the comet an account is given in the extensive monograph on the comet published by the Lowell Observatory.

4. But a third result was obtained by the unwitting collaboration of the spectrograms and the photographs. While the photographs were giving their pictures of the tail, the objective prism spectrograms were doing the like, with this difference that they recorded in a row pictures of it in the several colors of the spectrum, sifting out into a band those made by each separate wave-length of light. They thus made it possible to tell to what wave-lengths the visible appearances were due. For it became evident at once from the spectrograms that all wave-lengths were not equally concerned. On the contrary, there were in the spectral image several distinct tails with spectral gaps between. By an analysis of the wave-lengths yielding pictures of the tail was thus offered a diagnosis of the substances composing it. In this way it appeared that CO_2, carbon monoxide, was the chief constituent of the tail; CH_4, marsh gas, another; CN, cyanogen, a third component; and minute solid particles, giving a more or less continuous spectrum, a fourth.

That not one but a series of spectrograms was taken was important. It not only gave us a constitutional history of the tail but it showed the necessity of simultaneity in photographic and spectrographic observations for comparative purposes. For the series

demonstrated that the constituents of the tail varied markedly from one period to another. Thus from April 29, 1910, to May 7 the spectrum of the tail was almost wholly emissive. On May 11 it had changed to one nearly continuous, while on May 23 it had become largely emissive again and grew more so as time went on.

By comparing the photographic with the spectrographic series of representations of the tail a striking fact came to light. To appreciate this another point must be taken into account. In order to compare properly a photograph and a spectrogram, both should be made on the same brand of plate. No plate reproduces all parts of the spectrum with equal intensity. One kind of plate will emphasize certain rays and depreciate others; the next will reverse the estimation. Great error will then be introduced unless the plates be identical.

Now the photographs measured were taken with a Brashear 5-in. doublet, an excellent lens, on a Lumiere Σ plate. The rays registered by this plate extend from 3500 in the violet to 5160 in the green where the sensitiveness ceases. Indeed the effect would have stopped sooner had it not been for the hydrocarbon emission at this point. The light, therefore, of the photograph would be exactly differentiated into its constituents by a spectrogram taken on a Lumiere Σ plate. The only difference between the two would be due to the absorption of the objective prism, an absorption relatively greater for the violet than for the blue or green. This would work as much on one kind of light as on another of the same refrangibility and as the two different kinds we are considering, the emission and the continuous spectrum, are about equally spaced in the region of the violet, the correction needed on this account is small. We may, then, directly gauge the character of the photograph's light by that of the objective prism spectrogram taken on the Σ plate.

This we now proceed to do. On the exact date of the photograph no Σ plate was used with the two objective prisms though we have objective prism spectrograms on Cramer Iso. Instantaneous on May 23, 25, 26, 28 and 29. The nearest plate to the date in question was on May 29. Of this the best estimate gives for the constituents of the light of the tail at a distance of 3° to 6° from the head:

80 per cent. of emission bands of carbon monoxide,
20 per cent. continuous spectrum,

the hydrocarbon emission being, at this distance from the head too feeble to show.

Comparing now the spectrograms taken with a Voightlander lens and a Cramer Iso. Inst. plate on May 23, 25, 26, 28 and 29 we find that the ratio of the two kinds of light varied in the direction of relatively greater emission from the former to the latter date. On May 23 itself the plates are affected by moonlight so that a direct comparison of the relative ratios is too difficult to be made a basis of direct comparison, but that of May 28 gives for the ratios in the tail 3° from the head:

70 per cent. emission of carbon monoxide,
5 per cent. emission of hydrocarbons,
25 per cent. continuous spectrum.

Putting these facts together we shall not be far out of the way in stating the ratio on May 23 of the emissive and continuous spectrum of the tail at a distance of from 3° to 6° from the head for the Σ plate as

70 per cent. emission spectrum, CO and CH_4,
30 per cent. continuous spectrum.

We have then this interesting conclusion: that the knots which showed the action of a repulsive force exerted from the sun were chiefly composed, not of solid particles, but of *molecules of gases.*

5. To clinch this deduction I next turned to comet Morehouse. Catechized in this connection it not only corroborated the fact but emphasized it. Before the time of measuring the velocities in the tail of Halley's comet I had done the like for comet Morehouse, the knotted character of its tail offering promising inducement. I was not aware that Mm. Quenisset and Baldet, in France, and Professor J. A. Miller, of Swarthmore, Pa., had measured photographs of this comet in this manner previously and detected the same accelerated motion away from the head which my own later measures showed. My measures have also revealed why certain previous observers such as Barnard at Yerkes and Campbell at the Lick had failed to find such evidence.

Of comet Morehouse this observatory possesses about sixty negatives taken by Mr. E. C. Slipher. Among them are many pairs, the one plate following the other on the same evening. From the assortment thus offered I have selected two sets for measurement, the one a pair taken on October 31, 1908, at 8h 0m ± to 8h 42m ± M.S.T. and from 9h 14m ± to 10h 8m ± respectively; and the other a triplet on November 16, 1908, No. 1 being taken at 6h 25m to 7h 13m; No. 2 at 7h 24m to 7h 50m; No. 3 at 8h 0m to 8h 32m, respectively. I chose four knots on one and five on the other with the following results:

TAIL OF COMET MOREHOUSE, OCTOBER 31.

	Plate I., Distance Knot from Head.	Plate II., Distance Knot from Head.	Difference I. and II.
Knot 1	22′.8	24′.4	1′.6
Knot 2	72′.7	76′.2	3′.5
Knot 3	95′.5	99′.4	3′.9
Knot 4	128′.4	134′.6	6′.2

TAIL OF COMET MOREHOUSE, NOVEMBER 16.

	Plate I.	Plate II.	Plate III.	Diff. I.–II.	Diff. II.–III.
Knot 1	48′.7	49′.9	51′.4	1′.2	1′.5
Knot 2	63′.4	65′.4	66′.9	2′.0	1′.5
Knot 3	79′.7	81′.6	83′.5	1′.9	1′.9
Knot 4	87′.8	89′.3	90′.9	1′.5	1′.6
Knot 5	211′.1	215′.0	217′.7	3′.9	2′.7

6. It will at once be seen that both sets of plates show accelerated velocity in the particles of the tail away from the head as the distance from the head increases. In the first set the acceleration is fairly uniform, while in the latter the velocity does not increase until the distance out has become considerable. This affords the reason why some observers have failed to detect the motion. It is at times and in certain places masked. For this the following explanation may be offered: In the neighborhood of the head the several emissions are violently contorted as a mere inspection of the photographs show, and in consequence must be subject to collision with other portions of the tail. Possibly they encountered here matter in space which speaks unspeakably of motion other than that due solely to repulsive force. If now an observer chanced to make his measures

at this inopportune moment he would naturally conclude that no repulsion existed while in truth another motion was temporarily obstructing it.

7. Lastly the spectrograms and spectroscopic observations of Frost and Parkhurst, de la Baume Pluvinel and Baldet agree in showing the light of the tail of Morehouse's comet to have been due practically wholly to emission; in other words to glowing gas. Here, then, we have not only corroboration of the fact, brought forward from study of Halley's comet, to wit: that molecules of gas are repelled by the sun, but, from the light of the tail being composed solely of gaseous molecules, any supposition that they were not the cause of the visible effect, is entirely excluded.

We reach then this interesting conclusion: that molecules of gas not only may be but demonstratedly are repelled by the action of the sun and that though we have reason to suppose that minute solid particles may be similarly impressed it is of the former not the latter that we have direct proof at present.

LOWELL OBSERVATORY,
April 10, 1911.

THE NEW COSMOGONY.

By T. J. J. SEE.

(*Read April 21, 1911.*)

The results established in the writer's "Researches on the Evolution of the Stellar Systems," Vol. II., 1910, have given a new basis to our conceptions of the cosmogony. Instead of the traditional doctrine of throwing off, we now have that of capture, which means essentially that the nuclei originated in the distance and have since grown by accretion as they approached the centers about which they now revolve in greatly reduced orbits of small eccentricity. Not only have we witnessed a radical change in the point of view, but also in the method of research employed. And along with these changes has come the introduction of rigorous mathematical and dynamical criteria by which the mechanical principles involved may be extended over an almost unlimited period of time.

Not the least important of the improvements recently introduced is that resulting from a careful examination of the premises underlying our reasoning. Nothing is adopted from tradition, nor taken for granted, nor from any authority however high; but every question is examined on its merits and from the very ground up. As the subject is new it naturally follows that much still remains to be done; yet the general trend of nature's laws seems to be well established, and cosmogony begins to assume the form of a true science. Accordingly it may not be without interest to the general reader to summarize in one connected view the leading principles of the new science of cosmogony, with brief analysis of the criteria by which they are confirmed.

1. Babinet's criterion based on the mechanical principle of the conservation of areas, by which we are enabled to calculate the times of rotation of the sun and planets when expanded to fill the orbits of their attendant bodies, as imagined by Laplace. This enables us to say at once that the attendant bodies could never have

been detached by acceleration of rotation, as handed down by tradition from Laplace's original nebular hypothesis of 1796.

2. As the planets and satellites could not have been thrown off, they must have been captured and added on from without, or else have been formed from the agglomeration of fine dust right where they now revolve. This latter alternative, however, is easily shown to be impossible, owing to the feeble mutual gravitational attraction of small masses of matter under the stronger tendencies to dispersion by tidal action, which always exist near large centres of attraction. There remains therefore no possible mode of origin for the planets and satellites save that of capture, or addition to the system from without.

3. When first captured the satellites must therefore have been already of such considerable size that they were able to gather in, and consolidate with their globes, numerous smaller masses revolving in the vortices about the planets. The collisions arising in this process of the gathering in of smaller bodies by larger ones are strikingly illustrated by the craters noticed in the face of the Moon, which were formed by impact, the embedded satellites being in some cases at least twenty miles in diameter.

4. Thus while the satellites were all captured,[1] and were originally further from their planets than they are at present, they have grown larger in the course of ages as they revolved in the resisting medium about the planets, just as the earth and primary planets are still growing larger by the impact of meteorites against their surfaces, as they slowly approach the sun. The earth sweeps up daily 1,200,000,000 meteors, and the amount of this dust is calculated to to form a layer a millimeter thick in a century.

5. We know the satellites must have grown in mass since they were captured, because they have been drawn nearer and nearer their several planets, by increase of the central attraction, as in the celebrated problem of Gyldén.[1a] But if the mass of the sun has increased, by the downfall of cosmical dust, so also must the mass

[1] Since this was written the capture of Satellites has been independently confirmed by Professor E. W. Brown, in an important paper in the *Monthly Notices of the Royal Astronomical Society* for March, 1911, p. 453.

[1a] *A. N.*, 2593.

of the planet or satellite have been correspondingly augmented by the same cause.

6. For whilst the decrease of the major axis of the orbit of a satellite might result wholly from the growth of the mass of the planet and satellite, yet the decrease of the eccentricity of a satellite orbit can be explained only by collisions in the nebular resisting medium. This cause and no other whatsoever will explain the roundness of the orbits so characteristic of the solar system.

7. Accordingly as most of the satellites suffered collisions sufficient to reduce and well nigh destroy the eccentricities of their orbits,[2] it necessarily follows that all these bodies should have their surfaces indented by impacts with smaller masses, just as is shown by the craters on the moon.

8. For whilst Oppolzer, Gyldén and others have proved that the growth of the masses by the downfall of cosmical dust would increase the central attraction and bring the bodies close together, it is proved by the mathematical researches of Airy, Herschel, Lehmann-Filhés, and Strömgren, which I have carefully verified, that this decrease in the major axis does not decrease the eccentricity. Hence the decrease of the eccentricity is traceable to no cause whatsoever but the action of a nebular resisting medium, as held in my "Researches," Vol. II., p. 146.

9. The craters on the moon can therefore be due to no cause whatsoever other than the collisions which our satellite has suffered from other small bodies in space, and all divisions of opinion on the subject are henceforth swept away forever. For as the other satellites have had their orbits rounded up in nearing their several planets, it is necessary to suppose the same cause to have acted also on our moon, even if the eccentricity of the orbit in this case has not been rendered excessively small.

10. This solution of the problem of the roundness of the orbits—the leading problem in the cosmogony of our solar system—is what mathematicians call a *unique solution*. It reveals not only *a possible*, but also the *only possible cause of the extremely circular move-*

[2] In section 548 of his "General Astronomy," edition of 1904, the late Professor C. A. Young remarks that the "almost perfect circularity of the satellite orbits is not yet explained."

ment characteristic of the planets and satellites. The solution thus possesses all the rigor of a theorem in geometry, and meets the requirements of the most rigorous of the mathematical sciences.

11. The existence of planets beyond Neptune is indicated by the extreme roundness of Neptune's orbit; for this shows that the nebulosity was much too dense at that point for the system to terminate at the present known boundary. Moreover, as I have shown that the planets were originally connected with the comets, and the comets recede to their home in a spherical shell thousands of times the earth's distance from the sun, it necessarily follows that our planetary system extends on almost indefinitely. Several planets of considerable size must be assumed to revolve beyond Neptune, and they may yet be discovered by observation or photography, though at that great distance the practical difficulties will increase, owing to the feebleness of the sun's light and the slow orbital motion, which will require exposures of the photographic plate extending over many hours, and perhaps on successive days.

12. The planets have been built up out of cosmical dust, comets and satellites; so that all the matter now in the planets come originally from the heavenly spaces. This follows from the fact that the nebular development is from the outside toward the center, the formation always beginning in the distance and proceeding by accretion as the bodies gravitate towards the sun, and revolve in ever smaller and rounder orbits. This order of development is directly verified by the phenomena of the spiral and ring nebulæ; for here the movement is proved to be towards the center, where the sun develops for the domination of the system.

13. And just as our planets have been added onto the sun from without, not thrown off, as was erroneously taught for more than a century by Laplace and his successors, so also will similar planets have been formed by the same process about the other fixed stars. Thus there are undoubtedly systems of planets about the fixed stars, and they are habitable and inhabited like those revolving about the sun. Moreover, the other suns have their systems of comets, and their planets have captured systems of satellites as in our planetary system. This grand conclusion rests on an incontestable basis and is of transcendent philosophic interest.

14. The causes which have operated in the development of our solar system are thus general throughout the sidereal universe. Everywhere repulsive forces are dispersing fine dust from the stars to form the nebulæ, and the nebulæ in turn are settling down and whirling around to form stars with planetary systems about them.

15. Professor Barnard's magnificent photographs of the Milky Way show that cosmical dust everywhere pervades the heavenly spaces. And it is proved that variable stars are due chiefly to attendant bodies revolving in resisting media. When considerable bodies come into collision, as a large planet with a sun, the result is a temporary star or Nova.

16. The new cosmogony thus embraces within its scope the chief problems of the universe, and the dynamical causes assigned are deduced from simple phenomena operating according to known laws which are actually verified in the solar system. The arrangement of the nebulæ on either side of the Milky Way is the natural outcome of the operation of repulsive forces, the canopy of nebulæ congregating as far from the stratum of stars as possible. This assigns a known cause for the great order of nature first brought to light by the telescopic explorations of the elder Herschel in 1785.

Like astronomy itself it is obvious that cosmogony is at once the oldest and newest of the physical sciences. Having renewed its youth by the introduction of definite principles and exact methods, it has recently taken on such vigor that it promises to become the most majestic of the sciences. Nothing is more worthy of the attention of philosophers than the study of the great laws of the physical universe, and the marvelous processes of development by which the beauty and order of the cosmos came about. This was the great problem which gave rise to the development of the physical sciences among the Greeks, and it will always occupy a position of transcendent importance in the domain of natural philosophy.

U. S. Naval Observatory,
Mare Island, California,
April 3, 1911.

THE EXTENSION OF THE SOLAR SYSTEM BEYOND NEPTUNE, AND THE CONNECTION EXISTING BETWEEN PLANETS AND COMETS.

By T. J. J. SEE.

(*Read April 21, 1911.*)

One of the most remarkable results of the writer's recent researches on the origin of the solar system has consisted in the development of a satisfactory proof that the primordial nuclei of the planets were formed at great distances from the sun, and that their primitive orbits were highly eccentric like those now described by the comets; so that in the last analysis it is shown that the two classes of bodies are merged together, or rather that the planets have been built up by the agglomeration of cosmical dust, in the form of comets, and other fragments of matter, from our ancient nebula. The following is a brief outline of the thread of argument leading to this conclusion:

1. It is shown by the exact data supplied by Babinet's criterion that not one of our planets could have been thrown off from the sun, by acceleration of rotation, as imagined by Laplace in 1796, but that the nuclei must have started in the distance and since neared the sun, by insensible degrees, as the masses were gradually augmented by precipitations from the surrounding nebular medium.

2. When it was thus demonstrated by exact calculation that the premise handed down by Laplace is erroneous, our theory of planetary genesis was placed on a new basis by the proof that the roundness of planetary orbits is due to the secular action of a resisting medium, which has reduced the size of the planetary orbits and rendered them almost exactly circular.

3. In order to be so exactly circular, as they are now found to be, these orbits must originally have been very large, and also highly eccentric, like the orbits of comets; the orbits accordingly have been reduced in size by encounters with the other minor bodies, the

absorption of which also increased the masses of the planets enormously.

4. If one asks for ocular evidence that the planetary bodies have been in collision with smaller masses, this evidence is found in the phenomena shown in the face of the moon, which was formerly an independent planet, and is so small a globe as never to have developed water or atmosphere; so that it is a kind of hermetically sealed celestial museum, so near us in space that it serves for the illustration of the process of *absorption* and *capture* in cosmogony. The type of collisions visibly illustrated by the dents in the moon's face necessarily have occurred with all the planets; but the moon as our nearest planetary neighbor alone enables us to study the process of accretion by collisions with bodies of all sizes, from particles to satellites as large as twenty miles in diameter.

5. The obvious deposits of dust over the older lunar craters give them an aspect of great age, and in many cases the outlines of the craters are practically obliterated. In other cases newer craters are formed over the older ones; so that we can certainly infer by direct observation that the moon has been built up by accretion, dust being gathered in to be deposited over dust, and crater over crater. This is the same process which we see at work on the earth, except that the meteorites now swept up by our planet are generally small and consumed in the air before reaching the earth.

6. Since the planets were begun as independent nuclei in our nebula, and since augmented by the gathering together of an infinite number of small bodies, such as comets, the matter of planets and comets must necessarily be the same, for they are common products of our ancient nebula. The planets have been built up by the gathering in of satellites, comets and smaller particles of cosmical dust.

7. Now we have pointed out that Neptune's orbit is too round for it to be the outermost of the planets of the solar system. If the resisting medium was dense enough at that great distance to produce such extreme circularity in the motion of Neptune, there was enough of the nebulosity beyond that planet to make several more planets of comparatively large size. Thus it is certain that our system does not terminate at Neptune, but extends on almost indefinitely. It is probable that in time we may be able to discover

several trans-Neptunian planets; but the recognition of these remote bodies will be difficult, owing to their slow motion and the faintness of the sun's light at that great distance.

8. The notable expansion of our ideas of what constitutes a nebula will thus be of great practical use in the progress of astronomy. The overthrow of the theory of Laplace is only a small part of the service to science brought about by the discovery of the true laws of the development of our system. As the *comets* recede to distances amounting to thousands of times the earth's distance from the sun, so also must embryo *planets* be imagined to bridge over the gap heretofore separating the planets and comets. And we may imagine planets to extend to at least 100, perhaps 1,000 times the earth's distance from the sun. Some of the comets may go 100 times further yet, but at such great distances we can never know much about their motions in these remote regions of space.

9. When we contemplate the vast extent of our primordial nebula implied in the distances to which the comets recede, and remember the large apparent areas covered by many other nebulæ in the sky, we see that our solar nebula evidently was of the ordinary type, and that it certainly was not a gaseous mass in equilibrium under hydrostatic pressure and extending only to the orbit of Neptune. Of course all these old doctrines of Laplace are now quite abandoned, but they long deceived us, and kept cosmogony in a stationary condition for over a century.

10. The origin of the primordial nuclei in the distance is a necessary consequence of the working of planetary bodies towards the dominant center of attraction—the sun. Hence the formation of a system of planets is necessarily from without inward, just the reverse of the traditions handed down by Laplace. This harmonizes perfectly with the new theory of the spiral nebulæ, which makes the ring nebulæ particular cases of the more general spiral tendency. The formation in all cases is from the outside towards the center. Planets form in all nebulæ, and since small bodies approach the center more rapidly than large ones, under the action of a resisting medium, it follows that the planets thus capture systems of satellites such as we observe attending the planets of the solar system.

U. S. Naval Observatory,
March 7, 1911.

THE SECULAR EFFECTS OF THE INCREASE OF THE SUN'S MASS UPON THE MEAN MOTIONS, MAJOR AXES AND ECCENTRICITIES OF THE ORBITS OF THE PLANETS.

By T. J. J. SEE.

(*Read April 21, 1911.*)

In the days of Newton, Lagrange and Laplace, it was assumed that the formation of the planetary system was essentially complete, and the sun's attraction rigorously constant from age to age; and it was scarcely deemed necessary to consider the secular effects of slight modifying causes such as the downfall of cosmical dust upon the bodies composing the solar system. But the progress of the past century has shown that the Newtonian hypothesis of a constant mass and a central attraction depending wholly on the distance, but not on the time, is at best a very rough approximation to the truth; for in addition to the downfall of cosmical dust upon all the bodies of our system, it has been shown by the researches of Arrhenius, Schwartzchild and others, that the sun especially is losing finely divided matter under the action of repulsive forces such as we see illustrated in the streamers of the corona and the tails of comets. In our modern studies of the orbital motions of the heavenly bodies, therefore, we have to take the central mass as variable with the time, and consider the small secular changes which will follow from a variation of the central attraction incident to a gradual change of mass.

These questions have been treated in some form by many of the successors of Newton; and even this great philosopher himself in one case supposed that the central mass might be varied by a comet falling into the sun.[1] Laplace devotes considerable attention to the secular equations for determining the effects of the decrease of the sun's mass due to loss of light, then supposed to be of corpuscular

[1] "Principia," Lib. III., last proposition.

character.[2] The modern discussions based on the analytical methods of Gyldén are, however, much more satisfactory than those of the age of Laplace; and I propose to give a brief account of them, chiefly with a view of summarizing the state of our knowledge, and of removing some inconsistencies which may mislead those who are unfamiliar with the literature of the subject.

For example, in the late Professor Benjamin Peirce's "Ideality in the Physical Sciences," Boston, 1881, p. 131, the following curious statement occurs:

> The constant increase of the solar mass would have an influence on the planetary orbits. It would diminish their eccentricities, according to a law of easy computation. Hence it is possible that the orbits of the planets may have been originally very eccentric, almost like those of the comets; and their present freedom from eccentricity may have resulted from the growing mass of the sun. What modification of the nebular theory may be involved in this supposition cannot easily be imagined, without the guidance of some indication from nature.

This statement is misleading and erroneous, and the only way I can explain its appearance in the writings of Peirce is by the fact that his last lectures were prepared when he was at an advanced age and in ill health; and thus it is probable that some confusion occurred. Quite recently an analogous confusion has appeared in the *Astronomische Nachrichten*, No. 4454, in a short article by Dr. R. Bryant, on the secular acceleration of the moon's mean motion.

In order to place before the reader a summary of the chief investigations bearing on the problems now under discussion we cite the following papers:

1. "The Problem of the Newtonian Attraction of two Bodies with masses Varying with the Time," H. Gyldén (*A. N.*, 2593).

2. "Ein Specialfall des Gyldén'schen Problems," J. Mestschersky (*A. N.*, 3153 and 3807).

3. "Ueber Central Bewegungen," R. Lehmann-Filhés (*A. N.*, 3479–80).

4. "Note on Gyldén's Equations of the Problem of Two Bodies with Masses Varying with the Time," E. O. Lovett (*A. N.*, 3790).

5. "Ueber die Bedeutung Kleiner Massenänderungen für die Newtonsche Central Bewegung," Dr. E. Strömgren (*A. N.*, 3897).

[2] Mécanique Céleste, Liv. X., § 20.

The last of these papers is the most important, since it supplements and extends the results of the earlier investigators. Professor Strömgren's method is one of great generality and appears to be the most satisfactory yet devised; and we shall base our brief discussion chiefly on this paper.

If σ be a very small quantity, and $\phi(t)$ some function of the time, the original unit of mass becomes $1 + \sigma\phi(t)$, and the differential equations of motion become

$$\left.\begin{aligned} \frac{d^2x}{dt^2} + k^2[1 + \sigma\phi(t)]\frac{x}{r^3} &= 0, \\ \frac{d^2y}{dt^2} + k^2[1 + \sigma\phi(t)]\frac{y}{r^3} &= 0; \end{aligned}\right\} \tag{1}$$

where k^2 is the gravitation constant, and the mass is unity at the initial epoch $t = 0$.

The new constant of areas becomes

$$x\frac{dy}{dt} - y\frac{dx}{dt} = k\sqrt{[1 + \sigma\phi(t)]p} = \text{const.} \tag{2}$$

Other formulæ of interest are:

$$V^2 = k^2[1 + \sigma\phi(t)]\left(\frac{2}{r} - \frac{1}{a}\right), \tag{3}$$

$$\delta a = a_0\sigma\phi(t) - 2a_0^2\sigma\int_0^t \phi'(t)\frac{1}{r}dt, \tag{4}$$

$$\frac{1}{r} = \frac{1}{a}[1 + 2\Sigma J_i(ie)\cos i(e + nt - \pi)] \tag{5}$$

And finally after a careful investigation of all effects due to errors of the first order of the disturbing force, $\sigma\phi(t)$, Strömgren finds:

$$\left.\begin{aligned} \delta a &= -a\sigma\left[t + 2\frac{e}{n}(\sin E - \sin E_0)\right], \\ \delta e &= -\frac{1 - e^2}{n}\sigma(\sin E - \sin E_0), \\ \delta\pi &= \frac{\sqrt{1 - e^2}}{en}\sigma(\cos E - \cos E_0). \end{aligned}\right\} \tag{6}$$

Here n is the mean motion and E the eccentric anomaly. It will be seen from the first of equations (6) that the semi-axis major is diminished by a secular term depending on t, and by a periodic term depending on the difference of the sines of the angles E and E_0, or the position in the orbit. Thus the mean distance is subjected to both periodic and secular variation.

In the case of the eccentricity, however, the second of the equations (6) shows that there is no secular term, and only periodic changes occur. A similar remark applies to the longitude of the perihelion as shown by the third equation of (6).

We conclude, therefore, from Strömgren's careful analysis that there is no secular decrease in the eccentricity due to a steady growth of the central mass; and that the views expressed by Peirce and Bryant are due to confusion, or to some error in the chain of reasoning.

This conclusion accords with the result reached by Professor Lehmann-Filhés, in paper No. 3,[3] cited above. For Lehmann-Filhés shows that

$$\left.\begin{aligned} e\cos\pi &= e_0\cos\pi_0 + \text{periodic terms},\\ e\sin\pi &= e_0\sin\pi_0 + \text{periodic terms};\end{aligned}\right\} \qquad (7)$$

and remarks that when the attracting mass slowly increases the orbit slowly narrows up, but yet always remains a similar conic section. He adds that this is true for any eccentricity whatever. The results of Lehmann-Filhés and Strömgren, each worked out independently of the other, and with much detail, are therefore in entire accord; and as Strömgren's development is given in full, and every step in his analysis is quite clear, we must reject the conclusions of Peirce and Bryant as not well founded.

This conclusion that the *steady increase* of the central mass will not diminish the eccentricity also confirms the results reached by Airy[4] and by Sir John Herschel.[5] For these eminent authorities show that a central attractive disturbance decreases the eccentricity as the planet moves from the perihelion to the aphelion, but increases

[3] Cf. *A. N.*, 3479–3480.
[4] "Gravitation," pp. 50–51.
[5] "Outlines of Astronomy," tenth edition, 1869, p. 463.

it correspondingly in going from the aphelion to the perihelion; so that only periodic changes of the elements e and π occur.

Accordingly it follows that the only possible cause which could have diminished and practically obliterated the eccentricities of the orbits of the planets and satellites is the secular action of a resisting medium, as fully set forth in Volume II. of my "Researches on the Evolution of the Stellar Systems," 1910. Increasing the central mass accelerates the mean motions, and thus becomes very sensible in the theory of the motions of the planets; but it has no effect on the shape of their orbits. The almost circular form of the planetary orbits, therefore, may be referred to the secular action of a resisting medium and to no other cause whatsoever.

This result is of no ordinary interest, since it refers the roundness of the planetary orbits to but a *single physical cause,* and gives us what mathematicians call a *unique solution* of the leading problem of cosmogony. For Babinet's criterion shows beyond doubt that the planets never were detached from the central bodies which now govern their motions; and the argument given in Volume II. of my "Researches" proves that all these bodies were formed in the distance and afterwards neared the central masses about which they now revolve. The demonstration of the true mode of formation of our solar system is therefore supported by the necessary and sufficient conditions usually required in mathematical reasoning; and we may say that the laws of the formation of the solar system have been confirmed by mathematical criteria having all the rigor required in the science of geometry. This generalization will, I think, add not a little to our interest in the geometry of the heavens; and it is equally worthy of the attention of the astronomer, the geometer and the natural philosopher, who so long struggled to unfold the wonderful process involved in the formation of the planetary system.

U. S. Naval Observatory,
Mare Island, California,
March 20, 1911.

ON THE SOLUTION OF LINEAR DIFFERENTIAL EQUATIONS OF SUCCESSIVE APPROXIMATIONS.

By PRESTON A. LAMBERT.

(*Read April 20, 1911.*)

The object of this paper is to apply to the solution of linear differential equations, both ordinary and partial, the method of expansion into series used in the solution of algebraic equations in the papers read by the author before the Philosophical Society in April, 1903, and in April, 1908.

Let the given differential equation be

$$(1) \qquad f\left(x, y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \cdots, \frac{d^ny}{dx^n}\right) = 0.$$

The method of solution consists of the following steps:

(*a*) Break up the left-hand member of the differential equation into two parts,

$$f_1\left(x, y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \cdots, \frac{d^ny}{dx^n}\right)$$

and

$$f_2\left(x, y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \cdots, \frac{d^ny}{dx^n}\right),$$

such that the first part equated to zero can be integrated by some known method, and multiply the second part by a parameter S, independent of x and y. Replace the given equation by

$$(2) \quad f_1\left(x, y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \cdots, \frac{d^ny}{dx^2}\right) + Sf_2\left(x, y, \frac{dy}{dx}, \frac{d^2y}{dx^2}, \cdots, \frac{d^ny}{dx^n}\right) = 0.$$

(*b*) Assume that

$$(3) \qquad y = y_0 + y_1S + y_2S^2 + y_3S^3 + y_4S^4 + \cdots$$

makes equation (2) an identity.

(*c*) In this identity arranged according to the ascending powers of S equate to zero the coefficients of the different powers of S.

(*d*) Solve the differential equations thus obtained in regular order for $y_0, y_1, y_2, y_3, y_4, \cdots$.

(*e*) Substitute these values in (3) and make S unity. The resulting value of y, if it contains a finite number of terms or if it is a uniformly convergent infinite series, is a solution of the given differential equation.[1]

The method of solution of linear differential equations as here outlined does not seem to occur in mathematical literature except as developed by the author.

The method will be exemplified by applying it to two differential equations, important in mathematical physics—Bessel's equation, a second order ordinary differential equation, and Fourier's equation for the flow of heat, a second order partial differential equation.

Bessel's equation is

$$x^2\frac{d^2y}{dx^2} + x\frac{dy}{dx} + (x^2 - n^2)y = 0.$$

Replace Bessel's equation by

$$\left(x^2\frac{d^2y}{dx^2} + x\frac{dy}{dx} - n^2y\right) + Sx^2y = 0$$

and assume that

$$y = y_0 + y_1S + y_2S^2 + y_3S^3 + y_4S^4 + \cdots$$

makes the latter equation an identity.

When arranged in ascending powers of S this identity is

$$\begin{vmatrix} x^2\frac{d^2y_0}{dx^2} \\ + x\frac{dy_0}{dx} \\ - n^2y_0 \end{vmatrix} + \begin{vmatrix} x^2\frac{d^2y_1}{dx^2} \\ + x\frac{dy_1}{dx} \\ - n^2y_1 \\ + x^2y_0 \end{vmatrix} S + \begin{vmatrix} x^2\frac{d^2y_2}{dx^2} \\ + x\frac{dy_2}{dx} \\ - n^2y_2 \\ + x^2y_1 \end{vmatrix} S^2 + \cdots \equiv 0.$$

[1] This method gives a formal solution of non-linear differential equations, but up to the present time the author has been unable to test the resulting series for convergency.

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Equating to zero the coefficients of the powers of S in this identity, there result the following differential equations for the determination of $y_0, y_1, y_2, y_3, \cdots$.

$$x^2\frac{d^2y_0}{dx^2} + x\frac{dy_0}{dx} - n^2y_0 = 0,$$

$$x^2\frac{d^2y_1}{dx^2} + x\frac{dy_1}{dx} - n^2y_1 + x^2y_0 = 0,$$

$$x^2\frac{d^2y_2}{dx^2} + x\frac{dy_2}{dx} - n^2y_2 + x^2y_1 = 0.$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

The equation in y_0 is a homogeneous linear differential equation and its solution is

$$y_0 = Ax^n + Bx^{-n}.$$

Substituting this value of y_0 the equation for determining y_1 becomes

$$x^2\frac{d^2y_1}{dx^2} + x\frac{dy_1}{dx} - n^2y_1 = -Ax^{n+2} - Bx^{-n+2}.$$

This equation becomes exact when multiplied by x^{-n-1}. The resulting equation integrated gives a linear equation of the first order, the solution of which is

$$y_1 = \frac{-Ax^{n+2}}{2^2(n+1)} + \frac{Bx^{-n+2}}{2^2(n-1)}.$$

Substituting this value of y_1 in the equation for determining y_2 and proceeding in the same manner

$$y_2 = \frac{Ax^{n+4}}{2^4\cdot 2\,!\,(n+1)(n+2)} + \frac{Bx^{-n+4}}{2^4\cdot 2\,!\,(n-1)(n-2)}.$$

In like manner

$$y_3 = \frac{-Ax^{n+6}}{2^6\cdot 3\,!\,(n+1)(n+2)(n+3)} + \frac{Bx^{-n+6}}{2^6\cdot 3\,!\,(n-1)(n-2)(n-3)},$$

and so on.

Substituting these values of $y_0, y_1, y_2, y_3, \cdots$ in

$$y = y_0 + y_1 S + y_2 S^2 + y_3 S^3 + y_4 S^4 + \cdots$$

and making S unity,

$$y = Ax^n\left[1 - \frac{1}{n+1}\frac{x^2}{2^2} + \frac{1}{(n+1)(n+2)}\frac{x^4}{2^4\cdot 2!} - \frac{1}{(n+1)(n+2)(n+3)}\frac{x^6}{2^6\cdot 3!} + \cdots\right]$$
$$+ Bx^{-n}\left[1 + \frac{1}{n-1}\frac{x^2}{2^2} + \frac{1}{(n-1)(n-2)}\frac{x^4}{2^4\cdot 2!} + \frac{1}{(n-1)(n-2)(n-3)}\frac{x^6}{2^6\cdot 3!} + \cdots\right].$$

When n is not an integer the terms of both series in this value of y continue indefinitely according to the law of formation which inspection makes evident, both series are uniformly convergent except when $x = 0$, and both series are solutions of the given differential equation.

When n is a negative integer the law of formation of the terms of the first series changes after the (n)th term and when n is a positive integer the law of formation of the terms of the second series changes after the (n)th term. The second case will be considered.

When n is a positive integer the (n)th term of the second series is

$$y_{n-1} = \frac{Bx^{n-2}}{2^{2(n-1)}(n-1)!\,(n-1)!}.$$

Substituting this value of y_{n-1} in the differential equation for determining y_n,

$$x^2\frac{d^2y_n}{dx^2} + x\frac{dy_n}{dx} - n^2y_n + x^2y_{n-1} = 0,$$

and solving for y_n by the method used in solving for $y_1, y_2, y_3, \cdots$,

$$y_n = \frac{B}{2^{2n-1}n!\,(n-1)!}\left[x^n \log x - \frac{x^n}{2n}\right].$$

In determining $y_{n+1}, y_{n+2}, y_{n+3}, \cdots$, the second term in the bracket

gives the terms of the first series in the value of y multiplied by a constant. This new series is combined with the first series in the value of y.

The first term in the bracket gives

$$y_{n+1} = \frac{-B}{2^{2n-1}n!(n-1)!}\left[-\frac{x^{n+2}\log x}{2^2(n+1)} + \frac{x^{n+2}}{2^2(n+1)}\left(1+\frac{1}{n+1}\right)\right],$$

$$y_{n+2} = \frac{-B}{2^{2n-1}n!(n-1)!}\left[\frac{x^{n+4}\log x}{2!\,2^4(n+1)(n+2)}\right.$$
$$\left. - \frac{x^{n+4}}{2!\,2^4(n+1)(n+2)}\left(1+\frac{1}{2}+\frac{1}{n+1}+\frac{1}{n+2}\right)\right].$$

. . . . . .

The solution of Bessel's differential equation when n is a positive integer is therefore

$$y = Ax^n\left[1 - \frac{1}{n+1}\frac{x^2}{2^2} + \frac{1}{(n+1)(n+2)}\frac{x^4}{2^4\cdot 2!}\right.$$
$$\left. - \frac{1}{(n+1)(n+2)(n+3)}\frac{x^6}{2^6\cdot 3!} + \cdots\right]$$
$$+ Bx^{-n}\left[1 + \frac{1}{n-1}\frac{x^2}{2^2} + \frac{1}{(n-1)(n-2)}\frac{x^4}{2^4\cdot 2!}\right.$$
$$\left. + \cdots + \frac{x^{2n-2}}{2^{2(n-1)}(n-1)!(n-1)!}\right]$$
$$- \frac{Bx^n\log x}{2^{2n-1}n!(n-1)!}\left[1 - \frac{1}{n+1}\frac{x^2}{2^2} + \frac{1}{(n+1)(n+2)}\frac{x^4}{2^4\cdot 2!}\right.$$
$$\left. - \frac{1}{(n+1)(n+2)(n+3)}\frac{x^6}{2^6\cdot 3!} + \cdots\right]$$
$$- \frac{Bx^n}{2^{2n-1}n!(n-1)!}\left[\frac{1}{n+1}\left(1+\frac{1}{n+1}\right)\frac{x^2}{2^2}\right.$$
$$\left. - \frac{1}{(n+1)(n+2)}\left(1+\frac{1}{2}+\frac{1}{n+1}+\frac{1}{n+2}\right)\frac{x^4}{2^4\cdot 2!} + \cdots\right].$$

This is also the solution of the differential equation when n is a negative integer.

Fourier's partial differential equation for the linear flow of heat is

$$\frac{\partial V}{\partial t} = K \frac{\partial^2 V}{\partial x^2}.$$

Replace Fourier's equation by

$$\frac{\partial V}{\partial t} = SK \frac{\partial^2 V}{\partial x^2}$$

and assume that

$$V = V_0 + V_1 S + V_2 S^2 + V_3 S^3 + \cdots$$

makes the latter equation an identity.

When arranged in ascending powers of S this identity is

$$\left| \frac{\partial V_0}{\partial t} \right| + \left| \frac{\partial V_1}{\partial t} \right| S + \left| \frac{\partial V_2}{\partial t} \right| S^2 + \left| \frac{\partial V_3}{\partial t} \right| S^3 + \cdots \equiv 0.$$
$$\left| - K \frac{\partial^2 V_0}{\partial x^2} \right| \quad \left| - K \frac{\partial^2 V_1}{\partial x^2} \right| \quad \left| - K \frac{\partial^2 V_2}{\partial x^2} \right|$$

Equating to zero the coefficient of the powers of S in this identity, there result the following partial differential equations for the determination of $V_0, V_1, V_2, V_3, \cdots$,

$$\frac{\partial V_0}{\partial t} = 0, \qquad \frac{\partial V_1}{\partial t} - K \frac{\partial^2 V_0}{\partial x^2} = 0,$$

$$\frac{\partial V_2}{\partial t} - K \frac{\partial^2 V_1}{\partial x^2} = 0, \qquad \frac{\partial V_3}{\partial t} - K \frac{\partial^2 V_2}{\partial x^2} = 0,$$

. . . . . .

These partial differential equations solved in regular order give

$$V_0 = \phi(x), \quad V_1 = \phi''(x)(Kt), \quad V_2 = \phi^{IV}(x) \frac{(Kt)^2}{2!},$$
$$V_3 = \phi^{VI}(x) \frac{(Kt)^3}{3!}, \ \ldots.$$

Substituting these values of $V_0, V_1, V_2, V_3, \ldots$ in the assumed value of V and finally making S unity, there results

$$(A) \quad V = \phi(x) + \phi''(x)(Kt) + \phi^{IV}(x) \frac{(Kt)^2}{2!} + \phi^{VI}(x) \frac{(Kt)^3}{3!} + \cdots,$$

which is a solution of Fourier's equation for all values of $\phi(x)$ for which V either contains a finite number of terms or is an infinite series uniformly convergent both in x and in t.

The following table shows several values of $\phi(x)$ and the corresponding solutions of Fourier's equation,

I	(1) $\phi(x) = A$,	$V = A$,
	(2) $\phi(x) = Ax$,	$V = Ax$,
	(3) $\phi(x) = Ax^2$,	$V = A(x^2 + 2Kt)$,
	(4) $\phi(x) = A \sin(nx)$,	$V = Ae^{-n^2Kt} \sin(nx)$,
	(5) $\phi(x) = A \cos(nx)$,	$V = Ae^{-n^2Kt} \cos(nx)$,
	(6) $\phi(x) = Ae^{nx}$,	$V = Ae^{nx+n^2Kt}$,
	(7) $\phi(x) = Ae^{-nx}$,	$V = Ae^{-nx+n^2Kt}$,
	(8) $\phi(x) = Ae^{nx} \sin(nx)$,	$V = Ae^{nx} \sin(nx + 2n^2Kt)$.

It will be noticed that in these solutions $\phi(x)$ is the value of V when $t = 0$, that is $V = \phi(x)$ is the initial heat distribution.

It will also be noticed that in all these results x may be replaced by $x + a$. This statement is true of the results in the several following tables.

If Fourier's differential equation is replaced by

$$S\frac{\partial V}{\partial t} = K\frac{\partial^2 V}{\partial x^2}$$

and the assumption made that

$$V = V_0 + V_1 S + V_2 S^2 + V_3 S^3 + \cdots$$

makes this equation an identity, this identity arranged in ascending powers of S is

$$\left| K\frac{\partial^2 V_0}{\partial x^2} \right| + \left| K\frac{\partial^2 V_1}{\partial x^2} \right| S + \left| K\frac{\partial^2 V_2}{\partial x^2} \right| S^2 + \left| K\frac{\partial^2 V_3}{\partial x^2} \right| S^3 + \cdots \equiv 0.$$
$$\left| -\frac{\partial V_0}{\partial t} \right| \quad \left| -\frac{\partial V_1}{\partial t} \right| \quad \left| -\frac{\partial V_2}{\partial t} \right|$$

Equating to zero the coefficients of the powers of S in this identity,

$$\frac{\partial^2 V_0}{\partial x^2} = 0, \quad \frac{\partial^2 V_1}{\partial x^2} - \frac{1}{K}\frac{\partial V_0}{\partial t} = 0, \quad \frac{\partial^2 V_2}{\partial x^2} - \frac{1}{K}\frac{\partial V_1}{\partial t} = 0, \cdots.$$

Solving these partial differential equations in regular order for $V_0, V_1, V_2, V_3, \cdots$, substituting these values in the assumed expression for V, and finally making S unity, the result

$$(B) \qquad \begin{aligned} V = {} & \phi(t)x + \frac{1}{K}\phi'(t)\frac{x^3}{3!} + \frac{1}{K^2}\phi''(t)\frac{x^5}{5!} + \cdots \\ & + \theta(t) + \frac{1}{K}\theta'(t)\frac{x^2}{2!} + \frac{1}{K^2}\theta''(t)\frac{x^4}{4!} + \cdots \end{aligned}$$

is a solution of Fourier's differential equation for all values of $\phi(t)$ and $\theta(t)$ for which V either contains a finite number of terms or is an infinite series uniformly convergent both for x and for t.

Solutions of the differential equation when $\phi(t) = 0$ corresponding to several values of $\theta(t)$ are as follows—

II $\quad \phi(t) = 0,$

(1) $\theta(t) = A, \qquad V = A,$

(2) $\theta(t) = At, \qquad V = A\left(t + \frac{x^2}{2K}\right),$

(3) $\theta(t) = At^2, \qquad V = A\left(t^2 + \frac{x^2 t}{K} + \frac{x^4}{3 \cdot 4K^2}\right),$

(4) $\theta(t) = At^{\frac{1}{2}}, \qquad V = At^{\frac{1}{2}}\left[1 + \frac{x^2}{2!(2Kt)} - \frac{x^4}{4!(2Kt)^2} + \frac{3x^6}{6!(2Kt)^3} - \cdots\right],$

(5) $\theta(t) = At^{-\frac{1}{2}}, \qquad V = \frac{Ae^{-\frac{x^2}{4Kt}}}{Kt^{\frac{1}{2}}},$

(6) $\theta(t) = At^{\frac{3}{2}}, \qquad V = At^{\frac{3}{2}}\left[1 + \frac{3}{2!}\frac{x^2}{2Kt} + \frac{3}{4!}\frac{x^4}{(2Kt)^2} - \frac{3}{6!}\frac{x^6}{(2Kt)^3} - \cdots\right],$

(7) $\theta(t) = At^{-\frac{3}{2}}, \qquad V = At^{-\frac{3}{2}}\left[1 - \frac{3}{2!}\frac{x^2}{2Kt} + \frac{3 \cdot 5}{4!}\frac{x^4}{(2Kt)^2} - \frac{3 \cdot 5 \cdot 7}{6!}\frac{x^6}{(2Kt)^3} + \cdots\right],$

$$(8)\quad \theta(t)=A\sin(nt),\quad V=A\left[\sin(nt)+\frac{n}{K}\cos nt\,\frac{x^2}{2!}-\frac{n^2}{K^2}\sin(nt)\,\frac{x^4}{4!}-\cdots\right],$$

$$(9)\quad \theta(t)=A\log t,\quad V=A\left[\log t+\frac{1}{Kt}\,\frac{x^2}{2!}-\frac{1}{K^2t^2}\,\frac{x^4}{4!}+\frac{2}{K^3t^3}\,\frac{x^6}{6!}-\frac{2\cdot 3}{K^4t^4}\,\frac{x^8}{8!}+\cdots\right],$$

$$(10)\quad \theta(t)=Ae^{nt},\qquad V=Ae^{nt}\left[1+\frac{n}{K}\,\frac{x^2}{2!}+\frac{n^2}{K^2}\,\frac{x^4}{4!}+\cdots\right].$$

It will be noticed that in these solutions $V=\theta(t)$ is the heat distribution when $x=0$.

Solutions of the differential equation when $\theta(t)=0$ corresponding to several values of $\phi(t)$ are as follows:

III $\theta(t)=0$,

$$(1)\quad \phi(t)=A,\qquad V=Ax,$$

$$(2)\quad \phi(t)=At,\qquad V=A\left[xt+\frac{x^3}{3!K}\right],$$

$$(3)\quad \phi(t)=At^2\qquad V=A\left[xt^2+\frac{2x^3t}{3!K}+\frac{2x^5}{5!K^2}\right],$$

$$(4)\quad \phi(t)=At^{\frac{1}{2}}\qquad V=At^{\frac{1}{2}}\left[x+\frac{1}{2Kt}\,\frac{x^3}{3!}-\frac{1}{2^2K^2t^2}\,\frac{x^5}{5!}+\frac{3}{2^3K^3t^3}\,\frac{x^7}{7!}-\cdots\right],$$

$$(5)\quad \phi(t)=At^{-\frac{1}{2}},\qquad V=At^{-\frac{1}{2}}\left[x-\frac{1}{2Kt}\,\frac{x^3}{3!}+\frac{3}{2^2K^2t^2}\,\frac{x^5}{5!}-\frac{3\cdot 5}{2^3K^3t^3}\,\frac{x^7}{7!}+\cdots\right],$$

$$(6)\quad \phi(t)=Ae^{nt},\qquad V=Ae^{nt}\left[x+\frac{n}{K}\,\frac{x^3}{3!}+\frac{n^2}{K^2}\,\frac{x^5}{5!}+\cdots\right],$$

$$(7)\quad \phi(t)=A\sin(nt),\quad V=A\left[\sin(nt)x+\frac{n}{K}\cos(nt)\,\frac{x^3}{3!}-\frac{n^2}{K^2}\sin(nt)\,\frac{x^5}{5!}-\cdots\right],$$

$$(8)\ \phi(t) = A \log t, \quad V = A\left[x \log t + \frac{1}{Kt}\frac{x^3}{3!} - \frac{1}{K^2t^2}\frac{x^5}{5!} + \frac{2}{K^3t^3}\frac{x^7}{7!} - \cdots\right].$$

It will be noticed that in this set of solutions $V = 0$ when $x = 0$.

Let $u_1 = f_1(x, t)$, $u_2 = f_2(y, t)$, $u_3 = f_3(z, t)$ represent solutions of the three one-dimensional Fourier's equations,

$$\frac{\partial V}{\partial t} = K\frac{\partial^2 V}{\partial x^2}, \quad \frac{\partial V}{\partial t} = K\frac{\partial^2 V}{\partial y^2}, \quad \frac{\partial V}{\partial t} = K\frac{\partial^2 V}{\partial z^2}$$

respectively. It is readily proved that

$$V = u_1u_2 \text{ and } V = u_1u_2u_3$$

are solutions respectively of the two-dimensional Fourier's equation

$$\frac{\partial V}{\partial t} = K\left(\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2}\right)$$

and the three-dimensional Fourier's equation

$$\frac{\partial V}{\partial t} = K\left(\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2}\right).$$

This shows how solutions of the two- and three-dimensional Fourier's equations can be obtained from the solutions of the one-dimensional equation.

For example, from the one-dimensional solutions

$$V = \frac{Ae^{-\frac{x^2}{4Kt}}}{Kt^{\frac{1}{2}}} \quad \text{and} \quad V = Ae^{-n^2Kt}\sin(nx)$$

the three-dimensional solutions

IV (1) $$V = \frac{Ae^{-\frac{r^2}{4Kt}}}{K^3t^{\frac{3}{2}}},$$

(2) $$V = Ae^{-(\alpha^2+\beta^2+\gamma^2)Kt}\sin(\alpha x)\sin(\beta y)\sin(\gamma z),$$

respectively, are obtained.

If the solution of the three-dimensional Fourier's equation

$$\frac{\partial V}{\partial t} = K\left(\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2}\right)$$

is a function of r and t only, so that

$$V = f(r, t), \text{ where } r = (x^2 + y^2 + z^2)^{\frac{1}{2}},$$

the transformation of the given equation from rectangular to polar coördinates shows that the solution is

$$V = \frac{u}{r},$$

where u is a solution of the Fourier's equation

$$\frac{\partial u}{\partial t} = K\frac{\partial^2 u}{\partial r^2}.$$

It follows that solutions of the three-dimensional equation of the form $V = f(r, t)$ are obtained by replacing x by r in any solution of the one-dimensional equation

$$\frac{\partial V}{\partial t} = K\frac{\partial^2 u}{\partial x^2},$$

and dividing the result by r.

In this manner are obtained the solutions

V (1) $$V = \frac{A}{r},$$

(2) $$V = \frac{Ae^{\frac{-r^2}{4Kt}}}{rKt^{\frac{1}{2}}},$$

(3) $$V = \frac{A}{r}e^{nr}\sin(nr + 2n^2Kt).$$

It is interesting to compare the solutions of Fourier's partial differential equation obtained in this paper with the solutions tabulated by Sir William Thomson in the mathematcal appendix of the article on "Heat" in the "Encyclopaedia Britannica," ninth edition.

Sir William Thomson obtains his results by summation, that is

by integration, from the solution IV (1) above. All his results occur directly in the above tables or are combinations of two of these solutions. It is evident that there are several misprints in the results as printed in the "Britannica."

Of course there are many solutions of Fourier's equation which must be built up from elementary solutions, however found, by means of Fourier series, or which must be obtained by the methods of harmonic analysis.

The solution III (5) above is the series used by Sir William Thomson in his solution of the problem of the secular cooling of the earth.[2]

An interesting result in pure mathematics is obtained as follows: Sir William Thomson shows that for a continued point source of heat, if the rate is an arbitrary function of the time, $f(t)$, the solution of Fourier's equation when $K=1$ is given by the definite integral

$$V=\int_0^\infty dx f(t-x)\frac{e^{\frac{-r^2}{4x}}}{8\pi^{\frac{3}{2}}x^{\frac{3}{2}}}.$$

The second part of the general solution (B) above shows that

$$V=\frac{1}{4\pi}\left[\frac{1}{r}f(t)+f'(t)\frac{r}{2!}+f''(t)\frac{r^3}{4!}+\cdots\right]$$

is also the solution of Fourier's equation for the same conditions.

It follows that

$$\int_0^\infty dx f(t-x)\frac{e^{\frac{-r^2}{4x}}}{8\pi^{\frac{3}{2}}x^{\frac{3}{2}}}=\frac{1}{4\pi}\left[\frac{1}{r}f(t)+f'(t)\frac{r}{2!}+f''(t)\frac{r^3}{4!}+\cdots\right]$$

is a general formula for computing the definite integral.

LEHIGH UNIVERSITY,
BETHLEHEM, PA.

[2] "Mathematical and Physical Papers," Vol. III.

PROBLEMS IN PETROLOGY.

By JOSEPH P. IDDINGS.

(*Read April 21, 1911.*)

The development of the science of petrology from that condition of the study of rocks, properly termed petrography, is characterized by the shifting of the emphasis from the purely observational and descriptive phases of the work to those that relate to the origin and formation of rocks, both with respect to their occurrence as integral parts of the earth, and to their composition and structure.

Not that there is less need than formerly for accurate observation and study of rocks, and for thorough description of their composition, texture and occurrence, but the introduction of greater definiteness into conceptions of their modes of formation, and the widening of the horizon of this field of research through the experimental and synthetic investigations of the geophysicist, have advanced the study of rocks from the accumulation of data and statistics, to the formation of laws and relationships, both as regards minutest details of composition and texture, and with respect to petrographical provinces, and their connection with the dynamical history of the regions of the earth in which they occur.

As a consequence of this advance new problems present themselves, and invite the coöperation of workers in several branches of inorganic science. Leaving out of consideration for the present the great problems of metamorphism, some of which are being successfully treated by Adams, or have been under investigation by Van Hise and Leith, I wish to call your attention to certain phases of the study of igneous rocks that may be grouped under three heads for present purposes, as follows: (1) The actual mineral composition of igneous rocks, (2) the mathematics of the petrology of igneous rocks, and (3) petrographical provinces.

1. Actual Mineral Composition of Igneous Rocks.

Although the minerals constituting various rocks are their most obvious features, aside from their general color and texture, and have been the chief object of study by petrographers since the introduction of microscopical methods of investigation, they still remain among the most important problems before the petrologist.

The exact composition, crystal characters and optical properties of many of the minerals are well known. But some of the commonest, such as the micas, amphiboles and aluminous pyroxenes, are not perfectly understood chemically, and the relation between their composition and optical constants is not so definite that one may be employed to determine the other, as is the case with the lime-soda-feldspars.

Moreover, the exact amounts of the component minerals in various kinds of igneous rocks have not been determined, except in a very few instances; nor has the precise composition of those minerals that occur in mixed crystals, that is, the principal ferromagnesian minerals, been determined in the vast majority of the rocks described.

There is, therefore, a great field of research, imperfectly cultivated, capable of yielding immediate returns of the first importance for the solution of other problems connected with the mineral composition of these rocks.

Similarly, more definite and specific study and description are needed of the crystal forms and arrangements of the mineral constituents of igneous rocks than have heretofore appeared in petrography, in order that the texture of various rocks may be clearly understood, since texture is a very definite exponent of physical conditions that attended the crystallization of each igneous magma. Up to the present time petrographers have been content with very vague and incomplete descriptions of rock textures, as well as of kinds and amounts of minerals composing various igneous rocks.

The determination of the kinds and amounts of the minerals[4] in every rock leads to the problem of the formation of the minerals in each instance, and a comparison of the mineral composition of a rock with the chemical composition of the magma from which it

solidified. This involves the chemistry of solutions of inorganic compounds, chiefly silicates; the mutual interaction of the various chemical elements that appear in an analysis of the whole rock; together with the possible catalytic action of constituents, notably water gas, that may not become parts of the fixed compounds, but may escape in greater part upon the solidification of the magma.

Some of the minor problems, or factors, within this large one may be alluded to briefly as follows: The first and most obvious result of a strict correlation of the mineral composition of rocks with the chemical composition of the whole mass, representing the fixed components of the formerly liquid magma, is the recognition of the nonappearance in certain kinds of rocks of some minerals whose presence is necessary to satisfy the chemical requirements of the magma solutions. This is the case with completely crystallized but exceedingly fine-grained lavas of particular compositions, notably andesites.

Minerals that should be present to the extent of as much as 30 per cent. in some instances are not visible, are occult, and must

TABLE I.

	1.		2.	3.
SiO_2	59.87	quartz	13.02	18.29
Al_2O_3	15.02	orthoclase	17.24	9.46
Fe_2O_3	2.58	albite	28.56	24.63
FeO	3.40	anorthite	17.28	14.18
MgO	4.06	diopside	3.99	——
CaO	4.79	hypersthene	11.11	——
Na_2O	3.39	hornblende	——	15.39
K_2O	2.93	biotite	——	14.38
H_2O	1.86	hematite	——	1.12
TiO_2	0.72	magnetite	3.71	——
P_2O_5	0.26	ilmenite	1.37	——
F	0.02	apatite	0.50	0.50
Etc.	1.10	water	1.51	0.84
	1.00.00	Etc.	1.26	1.26
			99.51	100.05

be hidden within the substance of those minerals that are visible; that is, they must be held in solid solution within other kinds of crystals. An example will illustrate the case.

A magma whose chemical composition is shown by analysis 1,

Table I., under favorable conditions should form the mineral compounds in the proportions shown in column 2. In this there are 13 per cent. of quartz, and 17 per cent. of orthoclase, together making 30 per cent. of the whole. And this amount of quartz is the least amount of free silica capable of separating from a solution of such a chemical composition, assuming that the minerals formed are those known to occur in igneous rocks. A magma of this chemical composition commonly crystallizes as a pyroxene-andesite composed, so far as the microscope can determine, of lime-soda-feldspar, pyroxene, and magnetite, with no visible quartz or orthoclase. And, yet, from the chemical analysis of the rock there should be 30 per cent. of these compounds.

The orthoclase molecules may be readily imagined in solid solutions within the lime-soda-feldspars, although in coarsely crystallized forms of such a magma, diorite, orthoclase crystals appear as independent individuals. It has been shown in the Geophysical Laboratory of the Carnegie Institution. that orthoclase and anorthite molecules form homogeneous mixed crystals when melted together and cooled in an open crucible. The disappearance of 17 per cent. of orthoclase in this particular andesite is, therefore, due to the conditions of solidification of the rock. The non-appearance of the quartz may be explained in part by its existence in solid solution in other minerals of which, however, we have not sufficient evidence at present; or it may occur in minute crystals mistaken for andesine feldspar, since the optical properties of the two that may be recognized in minute crystals, are almost identical. In coarser grained forms of chemically similar magmas the quartz appears, but the conditions attending crystallization in the contrasted cases may favor its disappearance through solid solution in one instance, and its separation as quartz crystals in the other.

In this connection it is to be pointed out that the apparent actual mineral composition of certain igneous rocks may not be the real mineral composition by as much as 30 per cent. of the whole. For the occult minerals in solid solution are as much a part of the rock as though visible. Moreover, the percentages assigned to the minerals that are seen must be in error by the amounts of the occult

minerals in solution. The problem of the determination of the mineral composition of rocks is for this reason more complex than at first appears, and is further complicated by the difficulty of determining the amounts of colored and colorless crystals, when they appreciably overlap one another in thin section.

Another obvious result of a comparison of the actual mineral composition of igneous rocks with the chemical composition of their magmas is the notable variability in the combination of minerals that may in some instances result from the crystallization of magmas of like chemical composition. This is true both as to kinds and amounts of the resulting minerals. A striking illustration of this variability is found in the mineral composition of three rocks from the same region, the parish of Gran, Norway, which have been described by Brögger. Analyses of the three are shown in columns 1, 2 and 3, Table II. The first rock is an essexite, the second a camptonite, the third a hornblendite, and while the compositions differ slightly in percentages of silica, and to a less extent in other constituents, the chemical resemblances are striking, and the three analyses lie within the range of many well-known series of analyses of particular rocks.

TABLE II.

	1.	2.	3.	4.		5.	6.
SiO_2	43.65	40.60	37.90	42.35	orthoclase	7.2	6.1
Al_2O_3	11.48	12.55	13.17	12.29	albite	——	3.7
Fe_2O_3	6.32	5.47	8.83	3.89	anorthite	18.9	18.3
FeO	8.00	9.52	8.37	7.05	leucite	3.9	——
MgO	7.92	8.96	9.50	13.09	nephelite	11.1	10.2
CaO	14.00	10.80	10.75	12.49	diopside	26.9	30.2
Na_2O	2.28	2.54	2.35	2.74	olivine	8.0	17.9
K_2O	1.51	1.19	2.12	1.04	magnetite	11.6	5.6
H_2O	1.00	2.28	1.40	1.82	ilmenite	10.2	3.5
CO_2	tr	2.68	—— etc.	0.62	hematite	0.8	——
TiO_2	4.00	4.20	5.30	1.82	apatite	——	2.2
P_2O_5	tr	——	tr	.99	H_2O	1.4	1.8
	100.16	100.79	99.69	100.19	Etc.	——	0.3
						100.0	99.8

The first rock consists of lime-soda-feldspar and augite, with some olivine and mica, and rarely a little hornblende. The second rock consists of feldspar and hornblende in nearly equal proportions;

while the third is almost wholly hornblende, only 2 per cent. being pyroxene and nephelite. The same magma might have crystallized as nephelite-basanite, as appears from the calculated mineral composition shown in column 5, and from comparison with the analysis and mineral composition of a nephelite-basanite from Colfax County, N. M., shown in columns 4 and 6.

This is only an extreme case of variations well known to exist in most groups of rocks that may be referred to chemically similar magmas. And the magma already cited as capable of furnishing a pyroxene-andesite may also yield a quartz-mica-diorite, whose composition is shown in column 3 of the first table.

It is evident from these examples that the minerals called hornblende, or more properly amphibole, in the descriptions of these rocks differ widely in chemical composition, and often represent totally different mixed salts. Thus in the hornblendite of Gran, the hornblende contains all the components that might, under other conditions, have crystallized as pyroxene, olivine, feldspar, leucite, nephelite and magnetite.

Any attempt to correlate igneous rocks on the basis of the actual mineral composition, without taking into account the actual chemical composition of the minerals involved in each case must lead to confusion.

One of the most important problems in petrology is the elucidation of the laws controlling the production of mineral compounds from molten magmas. A consideration of the simpler chemical reactions that may be expected to take place in silicate solutions like rock magmas, and which do take place in crucibles in the laboratory, explains the formation of the feldspars, leucite, nephelite, quartz, diopside, hypersthene, olivine, magnetite and some other rock minerals.

Minerals like mica clearly involve the chemical action of water, or its components, hydrogen and hydroxyl, since hydrogen enters into its constitution. According to Penfield hydroxyl, and sometimes fluorine, enters into the composition of hornblendes, forming bivalent radicles with aluminium, and ferric iron. In pyrogenetic analcite, and in other possibly primary zeolites in igneous rocks, H_2O enters

PROC. AMER. PHIL. SOC. L. 199 S, PRINTED JUNE 30, 1911.

into the silicate compound. The physical conditions which control the chemical equilibrium within magma solutions that yield these mineral compounds are problems for the geophysicist, though their nature may be inferred in a general way from the mode of occurrence of the rocks containing the minerals in question.

Indications of a catalytic action of H_2O within rock magmas are furnished by the association of free silica with orthosilicates containing magnesium and iron, such as the common occurrence of quartz and biotite in granitic rocks; the frequent association of quartz and tridymite with olivine in lavas; and of quartz, tridymite and fayalite in lithophysae in certain highly siliceous lavas.

The instability of these systems under changed conditions of equilibrium is shown by the inversion of hornblende to an aggregation of pyroxene, magnetite and feldspar, in some lavas; and by the solution of quartz phenocrysts in some basalts, accompanied by the formation of shells of metasilicates surrounding them.

Already laboratory research has established the range of stability of some of the rock minerals under laboratory conditions: the inversion temperatures under atmospheric pressures of the various forms of SiO_2, quartz, tridymite, crystobalite; of the simpler compounds crystallizing as orthorhombic and monoclinic pyroxene, and the corresponding amphiboles; of a simple system involving aluminium, magnesium calcium silicates; and of other series of compounds. The value of these definite contributions to the problems of the mineral composition of igneous rocks is great. Much more is needed. And the necessity for eventually approaching nearer to the physical conditions obtaining in rock magmas is apparent, when the probable efficiency, chemical and physical, of highly heated gases under strong pressures is taken into consideration. Research under such conditions is attended with great difficulties, and some risks. Enough has been mentioned to show a wide range for future study by the geophysicist, the chemist, and the petrographer.

2. The Mathematics of the Petrology of Igneous Rocks.

The study of igneous rocks involves the consideration of groups of intricate relationships, the exact expression of which is at pres-

ent beyond our competence. Abstract conceptions of some of the simpler relationships, based on partial knowledge of the factors involved, serve to point the way along which quantitative investigation may be profitably pursued.

The stoichiometric character of the chemical compounds that constitute rock minerals relates them as definite functions to the chemical constituents of the liquid magma from which they crystallized. The existence of mixed crystals, and of solid solutions, introduces the treatment of series into the problem of the expression of the relationship between the mineral composition of a rock and the chemical composition of its magma. In such an expression the fixed components alone are involved. But there are definite quantitative relationships to be expressed regarding those chemical components of a magma which may act only catalytically in producing the actual mineral combination constituting the rock. Such actions may be chemical, in the sense that compounds form that subsequently disappear, as should H_2O combine with SiO_2 to form hydrogen orthosilicate, H_4SiO_4, and subsequently resolve itself into water and quartz or tridymite. Or they may be physical, in the sense that increased molecular mobility in the magma liquid may affect the character of the crystallization by changing the freezing point and the nature of the compounds stable under the conditions obtaining at the time. In the broadest sense, then, the mineral composition of an igneous rock is a function of the chemical composition of the magma.

Since the physical conditions attending the solidification of rock magmas affect the chemical equilibrium of the constituents, as well as the physical character of the liquid, its temperature and viscosity, and also influence the chemical composition with respect to the gaseous components capable of being held in solution under pressure, the mineral composition of an igneous rock is also a function of the physical conditions attending its solidification.

To a notable extent is this also true of the texture of such rocks, their degree of crystallization, size of grain, and the shape and arrangement of the individual minerals. In the expression of these relationships the treatment of serial functions must be a pronounced feature. The gradual variations of temperature and pressure are as

essential factors in the consideration of the physical conditions of rock magmas, as the variations in texture and in mineral composition are universally characteristic features of igneous rocks.

The existence of definite quantitative relations between the mineral composition and the texture of igneous rocks on the one hand, and the chemical composition of the magma and the physical conditions attending its eruption and solidification on the other, rests on the obedience of the component elements to the laws of physical chemistry. These laws are not fully established, or known, at this time, and the relationships involved may be too intricate to be completely expressed in customary mathematical terms, nevertheless, the definiteness of the quantitative relationships can not be doubted, and approximate expressions of them become problems for petrologists of the future.

In the consideration and correlation of all known igneous rocks, variability in composition and texture and the existence of continuous series are the most conspicuous general characteristics. The variability in the composition of igneous rocks indicates heterogeneity in magma solutions. This may be inherent in them, and represent a condition of existence before the initiation of eruption; or, as is more probably the case, it may result from differentiation of homogeneous magmas during periods of eruptive activity, within more or less extended regions. Differentiation results from diffusion of compounds in solid molecules, or less complex ones, either at the time of separation as crystals, or earlier, through convection currents, differences in density, or differences in solution pressure. The resulting magma solutions differ only in the quantities of various chemical compounds; the amount of some in extreme instances reaching zero. Subsequently formed compounds are not inherently different from those in other magmas except by reason of the amounts of certain chemical components, which may be concentrated in some differentiated parts; as in the concentration of the rare elements in some pegmatites; or by different combination of chemical elements through catalytic agents. There are no inherent, or inherited, characteristics of form, organism, or immaterial traits, as in living beings. The magmas are simply differently mixed solutions of inorganic compounds.

Magma solutions possess different degrees of heterogeneity as shown by the composition of various bodies of igneous rocks. In some there are slight differences in different parts, extending through large masses. In others marked differences occur within short distances in small masses. Variability in the composition of igneous rocks from place to place is a universal characteristic, resulting in series of varieties of composition within single bodies, and among different masses. The aggregate of all such series of variations in one region may form a continuous series of wide extent; or there may be gaps in the series in one region, which may be filled by the phases of composition exhibited by rocks in another region.

In one region the composition of a nearly homogeneous rock mass of considerable magnitude may assume a certain local petrographic importance, while in another region it may appear only as a facies of another rock body. There appears to be no chemicophysical reason for the production of a magma solution of one mixed composition rather than of another very nearly the same. But it is known that magmas of intermediate, or more mixed, compositions, are more abundant than those of extreme, or simpler, compositions.

The accumulated evidence of chemical analysis, microscopical study of rock sections, and observation in the field, shows the existence of wide serial variations of composition, continuous along numerous lines, owing to the number of variable components. This evidence also shows that there is no one definitely composed magma solution more abundant throughout large areas of the earth than others; none that deserves special consideration, or may be recognized as a universal type. It is true, as already remarked, that in certain regions there are large bodies of rock having nearly uniform composition that assume local importance, and serve as types for reference in particular regions. But it must be admitted that the idea of type is subjective, inherent in the petrographer, not the rock. And when all known series of igneous rocks are treated as products of chemicophysical reactions universal in their application, the fortuitous character of the chemical composition of particular bodies of erupted magma becomes apparent, and the significance of such local types disappears in a systematic treatment of the whole body of

petrographical facts involved in a comprehensive description of igneous rocks.

Recognizing the existence of continuous series of petrographical factors, chemical, mineral and textural, necessary to the complete description and definition of igneous rocks, the problem presents itself of dividing the complex series of rocks so characterized into parts that may be described in a comprehensive and systematic manner.

A familiar example of a physical series divided in a regular manner for purposes of exact use is that of temperature, partitioned in degrees of definite proportions of a continuous scale. It is undoubtedly an arbitrary method and differs distinctly in three commonly employed usages. It might be a more "natural" method to express temperature with reference to the melting points of a series of substances; and the value of certain of these definite points as datum points is well known. But the merits of the arbitrarily, but very naturally, divided scale are attested by its universal employment.

The proposal to partition the petrographical series into quantitatively definite parts, as has been done in the Quantitative System of Classification of Igneous Rocks, the size of the divisions being arbitrarily chosen, has excited criticism by some petrographers, who consider it arbitrary, artificial and not "natural." But the objection, that measured precision condemns a classification of igneous rocks, because it makes evident "its aloofness from the scheme of nature based not on arithmetical but on physical and chemical principles,"[1] suggests a lack of appreciation of the mathematical precision of stoichiometric chemistry, and a failure to grasp the definiteness of quantitative physics, whose natural expression is found in higher mathematics. Both of these sciences are fundamental to that of petrology; and as mathematics is the language, or expression, of quantitative relationships, the more definite the knowledge of the quantitative factors and relationships obtaining in igneous rocks, the more natural will become their expression in mathematical terms.

Acknowledging the usefulness of such terms as "consanguinity" and "parent" magmas, in emphasizing the fact that there is relationship between rocks in certain instances, it must be admitted that the too frequent use of these and other biological terms, as "families"

[1] Harker, A., "The Natural History of Igneous Rocks," 1909, p. 366.

of rocks, minerals of "first and second generation," and the like, tends to convey by implication the idea that there exists among igneous rocks genetic relationships analogous to those sustained by living organisms. In fact, this idea has been clearly formulated by Harker[2] in stating that the mutual relationships of igneous rocks will furnish a "fundamental principle analogous with that of descent, which lies at the root of natural classification in the organic world."

The significance of the term "natural" when applied by some petrographers to petrographic classification appears to be pregnant with biological conceptions. But what is proper and natural in the treatment of assemblages of organisms is not for that reason, necessarily, proper, or natural, in the treatment of a series of chemical solutions and their solidified phases, however much the various solutions may be related to one another by reason of differential diffusion or fractional crystallization.

3. Petrographical Provinces.

Although the fact has been recognized for twenty-five years that there are regions within which the rocks erupted during any particular period exhibit certain peculiarities of mineral composition and texture that distinguish them from rocks belonging to the same general group, erupted simultaneously in other regions,[3] little or no attempt has been made to define more precisely what constitutes the characteristics of any so-called petrographical province.

It has been pointed out that in some regions many of the igneous rocks are especially rich in alkalies; in some sodium being prominent; in others potassium. But nothing approaching completeness of definition, either as to composition of the rocks, or extent and limit of the region of occurrence, has ever been attempted.

And yet some very general and far-reaching speculations have been indulged in on the basis of hastily formed impressions, both as to the character of such groups of rocks and their relationship to assumed structural features of the earth. As a result certain petrographers have grouped all igneous rocks into two contrasted cate-

[2] *Ibid.*, p. 362.

[3] Judd, J. W., *Quar. Jour. Geol. Society*, London, 1886, Vol. 42, p. 54.

gories, without considering the probability of their being many phases of combination of the variable factors of igneous rocks that must characterize all petrographical provinces of the earth.

The assumption that rocks must either belong to what have been called the "Atlantic" or the "Pacific" provinces, without serious definition of either of these rather comprehensive terms, has led to the humorous conclusion that the igneous rocks of Great Britain belonged in some periods of geological history to the "Atlantic," in others to the "Pacific" provinces; indicating the flexible, one might say caoutchouc-like, nature of these provinces.

The igneous rocks of the Andes and of the western Cordillera of North America have been referred to as representing the "Pacific" province, while the more alkalic rocks of Scandinavia and of some other parts of Europe are considered to represent the "Atlantic." The igneous rocks of Great Britain belong to neither of these distinctive groups as a whole. And the rocks erupted at different geological periods in Great Britain, while they exhibit some variations in extremes of composition, which might result from different degrees of differentiation of chemically similar magmas, bear some of those resemblances to one another that are supposed to characterize rocks of one petrographical province.

The misconception underlying the generalization responsible for the terms "Atlantic" and "Pacific," as applied to petrographical provinces, appears from the facts brought out by Cross regarding the alkalic character of some of the lavas of Hawaii, and by Lacroix regarding alkalic rocks in Tahiti; to say nothing of similar rocks in New Zealand and elsewhere in the southern Pacific. Moreover, in the midst of Europe, in Hungary, there are groups of rocks identical in all respects with those of the Great Basin in western America.

From this it is evident that one of the most important and interesting problems before petrologists is the investigation and exact definition of the districts and regions of igneous rocks in all parts of the world, with the purpose of obtaining the data with which to form definite conceptions of what have been termed petrographical provinces. Enough is known already to make it evident that there are many kinds of such groups of igneous eruptions and not two strongly contrasted series; that they blend into one another in composition;

that the delimitation of the regions, or provinces, may be pronounced in some instances, and ill-defined in others.

The character of the rocks in different provinces, and the distribution of provinces throughout the earth, together with their relations to the geological structure and dynamical history of the region in which they occur, furnish problems of the first magnitude in petrology.

One of the questions to be answered is: the relation of the composition of igneous rocks of different parts of the earth to its isostasy. The configuration of the earth's surface demands the presence of material of different densities beneath the surface. Does this show itself in the character of the material erupted in different regions. An answer to this can not be given offhand. The requirements in density are relatively so slight when great volumes are concerned, as pointed out recently by Hayford;[4] the series of igneous magmas of any region is so diversified in composition and density; and the estimation of their several volumes is so hazardous an undertaking that a reasonable solution of the problem can only be expected after the accumulation of a great amount of exact data.

Whether there is any relation between the kinds of magma erupted in a particular region and the dynamical events within the region is another problem yet to be solved. Assertions to the effect that there is a definite relationship have been made, but they are in the nature of broad generalizations upon questionable premises, producing the results already discussed in connection with the terms "Atlantic" and "Pacific."

It is possible that differences in the sequence of dynamic events in various regions, or in one region at various periods of its history, may be accompanied by differences in the processes and results of differentiation of chemically similar magmas; that is, in series of erupted rocks, but the existence of such relationships has yet to be clearly established. For it is also possible that the material of the earth may be heterogeneous in composition, differing somewhat from place to place, and yielding different kinds of magmas in different

[4] Hayford, J. F., "The Relations of Isostasy to Geodesy, Geophysics and Geology," *Science*, N. S., Vol. 33, No. 841, 1911, pp. 199–208.

regions, each of which may undergo local differentiation according to conditions of its eruption. The apparent persistency of the major features of relief on the earth's surface and the demands of isostasy suggest an absence of homogeneity within the material of which it is composed. The solution of these fundamental problems in geology must rest on petrological research along the lines here indicated.

Such are some of the more obvious problems of petrology, the solution of which involves the coöperation of petrographers with the chemist, the geophysicist and the geologist.

A STUDY OF THE TERTIARY FLORAS OF THE ATLANTIC AND GULF COASTAL PLAIN.[1]

By EDWARD W. BERRY.

(*Read April 21, 1911.*)

Introductory.

The observations recorded in the following pages may be said to represent a preliminary sketch of a small chapter in the study of the South Atlantic and Gulf Coastal plain undertaken by the United States Geological Survey in coöperation with the various state surveys under the direction of Dr. T. W. Vaughan.

Neither geologist nor biologist fully appreciates the magnitude, complexity or uniqueness of the coastal plain of the southeastern United States. The present coast line, a boundary first recognized by the aborigines and early explorers and so emphasized by geographers, is from the standpoint of the student of geologic history a continually shifting demarcation which does not, nor perhaps never, marked the seaward limit of the physiographic unit known as the Coastal Plain Province, for the gently sloping land surface continues seaward beneath the waves of the present Atlantic and Gulf waters varying distances up to 100 miles or more and then precipitately descends several thousand feet in a few miles, forming the majestic escarpment which is regarded as the continental boundary. In the past the coast line has advanced inland over the present emerged portion of the coastal plain and receded seaward over the present submerged margin, many times. At one time the waves of the Gulf of Mexico broke in southern Illinois, at another they were confined 100 miles south of the present sites of Mobile and New Orleans, 600 miles to the southward.

On the whole, the history of events in Tertiary times has been a progressive adding to the land area of the continent, the most im-

[1] Published with the permission of the director of the United States Geological Survey.

portant elevation being that of the early Miocene which was followed by a subsidence, which was, however, less in extent than those which had preceded it.

No part of the coastal plain is so favorably situated for the study of the floras which preceded the present, extending backward to a time which marks the first recorded appearance of angiosperms, as that of the Gulf states. No single part of North America contains so continuous a series of Tertiary deposits carrying fossil plants. Here we find abundant floras in the lower and middle stages of the Eocene, considerable floras in the Oligocene, some in the later Miocene, and rather abundant fossil plants in the Pliocene. The Rocky Mountain region is rich in Eocene fossil plants and there are some Miocene floras, but no Oligocene or Pliocene floras are known. The Pacific coast region likewise furnishes Eocene and Miocene fossil plants but none of Oligocene age. The fossil floras of the coastal plain are found in an area where it is possible to attain to some measure of accuracy in predicating the general character and course of ocean currents and winds and other physical features of the environment. On the other hand the western floras just mentioned grew in areas where vulcanism was great at times; in areas of great orogenic activity, where changes in topography were numerous and elevations of several thousands of feet are recorded; areas in which climatic conditions not only varied from place to place, but passed through a large cycle of secular changes. All these factors greatly complicate the floral history.

The floras of the southern coastal plain are moreover checked for the most part by very abundant marine faunas in intercalated beds, or the plant-bearing beds which represent the coastal swamps and the shallow water deposition of the old embayment merge laterally with the contemporaneous limestones or marls which were forming in more open waters along the coasts to the southward, so that there is a considerable body of facts bearing on depth, character of the bottom, and marine temperatures, with which to compare land temperatures. These criteria have been admirably worked out for the Florida area by Doctors Dall and Vaughan for the post-Eocene and their results furnished a reliable datum plane for the deductions to be derived from the study of the fossil floras of these times.

So far as I know I was the first paleobotanist to explore the south Atlantic and Gulf coastal plain and that exploration has only just begun. Professors Fontaine and Ward visited the region and collected a few Cretaceous plants a score of years ago. Professor Lesquereux a generation and a half ago described a few Eocene plants collected by Professor Hilgard in Mississippi and by Professor Safford in Tennessee, and Doctors Knowlton and Hollick have identified various small collections made by others in different parts of this vast area.

With the exception of fragments of the petrified stems of conifers, palms and dicotyledons the plant-remains are in the form of impressions, mostly of foliage, but with a goodly sprinkling of fruits and seeds, and in some few cases even flowers are preserved.

While the oscillations of the Gulf area have been numerous they have been, as I have just mentioned, inconsiderable in amount, only a few hundred feet at most, and the coastal region has uniformly been one of slight relief. The various floras show a complete absence of upland types. This is in striking contrast to the European older Tertiary floras. The only large area of the globe which has been thoroughly studied, Europe, was far less stable than this region in Tertiary times and lying much farther toward the pole was subjected to the rigors of Pleistocene conditions whose influence never reached our southern states.

The object of the writer's work may be classed under three heads: (1) To determine the correlation of the various Tertiary formations particularly in the upper portion of the Mississippi embayment where marine fossils are largely absent, (2) To obtain data regarding the physical conditions under which the various floras flourished, (3) To accumulate biological data regarding the geographical distribution, specific differentiation and evolution of the Tertiary floras.

Thus one of the principal phases of the study for the geologist might be embraced under the term paleoecology. The methods include a study of the old shore lines of the different epochs, of the character of the sediments and their genesis, of the contained animals and plants, and the alternative climatic and edaphic factors which their grouping may indicate.

It is the chronologic and ecologic aspects upon which I wish to dwell in the present connection.

The paleobotanical record of the Atlantic and Gulf coastal plain furnishes a history which extends back as I have just mentioned beyond the oldest known angiosperm to a time (Lower Cretaceous) when the flora was made up almost entirely of tree-ferns, conifers and those interesting cycadophytes (*Cycadeoidea*) whose trunks are sometimes preserved with such marvelous perfection that the outlines of the embryos in the ovules can often be made out in detail. Coming a step nearer my present theme, a step of some millions of years from the Lower into the Upper Cretaceous we find the first great modernization of the floras of the world due to the seemingly sudden evolution of the main types of angiosperms. These upper Cretaceous floras are well represented in the coastal plain from Marthas Vineyard to Texas. They extended northward to Greenland and southward to Argentina in South America, and are found to indicate very different physical conditions from those which prevail at the present time. I do not intend, however, to dwell upon the Upper Cretaceous floras in this connection but pass to a consideration of the succeeding Eocene stage of plant evolution. In this as in subsequent times the chief emphasis will be laid upon that section known as the embayment or old Mississippi Gulf, although where the record is more complete in other parts of the coastal plain I will not hesitate to use it.

Basal Eocene.

The Eocene as defined by Lyell was marked by the dawn of the recent species of marine mollusca. It is equally well marked by the sudden expansion and evolution of modern types of mammals and plants after a long antecedent Cretaceous development. The floras become thoroughly modernized as compared with those which preceded them, although they are still very different in their general facies and distribution from those of the present.

In the earliest stage of the Eocene known as the Midway, the relations of sea and land in the Gulf area differed in only minor particulars from that of the late Cretaceous. The waters of the Missis-

sippi Gulf were, however, deeper. This factor combined with a much less influx of fresh water from the tributary streams, due in some measure to the low relief of the land, enabled marine faunas to reach well toward the head of the gulf. These faunas indicate subtropical bottom temperatures northward as far as Paducah, Ky. The known floras are very scanty and unsatisfactory and in the present state of our knowledge do not merit an extended discussion.

Lower Eocene.

The Midway Eocene was succeeded by a long interval during which a great thickness of deposits was laid down which are collectively known as the Wilcox Group. The character of these sediments and their faunas show that the gulf was somewhat restricted and much shallower than in the preceding stage, with true marine conditions prevalent only in its lower portion. The shores were low and relatively flat. They were flanked by current- or wave-built bars and separated from the mainland by shallow inlets or lagoons. The lower courses of the streams were transformed into shallow estuaries or broad swamps through which the smaller streams meandered. The accompanying sketch map (Fig. 1) shows the relation of land to water at this time. The shore line along which the strand flora migrated is approximately indicated, and some of the localities where fossil plants have been discovered in the littoral deposits of this age are indicated by stars, while the general movement of the warm ocean currents is indicated by arrows. A magnificent flora is preserved at a large number of localities in the clay lenses which were formed in these estuaries and marginal lagoons. This flora shows a mingling of tropical and subtropical types as far northward as where the Ohio River now flows into the Mississippi. It is of unparalleled richness and preservation and will bear a more extended analysis.

Among the ferns it contains representatives of the genera *Acrostichum, Pteris* and *Lygodium,* none of which appear to be common. Both feather and fan palms are not uncommon. Conifers are represented by a single occurrence of a species of *Arthrotaxis*—a genus which in the living flora is confined to the coastal swamps of Tas-

mania but which is widespread in European Eocene floras. A large variety of dicotyledonous forms are preserved, representatives of about two hundred different species of which about one third have thus far been satisfactorily identified. These include seven or eight species of leguminous shrubs and trees represented by pods as well as leaflets—evidently strand plants, as are numerous modern species

FIG. 1. Sketch map showing the approximate relation of land to water in the Lower Eocene. Stars indicate fossil plant localities, diagonal lining indicates submerged areas.

of *Acacia, Cæsalpinia* and *Dalbergia*. Evergreen lauraceous forms are also abundant, the genera *Cinnamomum, Laurus, Malapoenna, Persea, Oreodaphne* (*Ocotea*), etc., being represented by several species. Figs are abundant and of several species, embracing both

the pinnately veined and the palmately veined types. There are three or four species of *Sapindus*—another strand type of the modern equatorial and subequatorial zones. Other members of the strand flora include representatives of the genera *Conocarpus, Guetteria, Mimusops, Persoonia, Terminalia,* etc. Leaves of several species of live oaks (*Quercus*) are abundant. The collections also include fruits of the families Anacardiaceæ and Umbelliferæ, and of the genus *Aristolochia.* Curious elements common to Europe are several species of *Banksia,* an antipodean genus in the existing flora. There is a fine species of *Cercis,* a very common *Euonymus* and at least two species of *Engelhardtia* based upon the characteristic fruits as well as leaves. The latter genus has a single existing species in Central America and several in Asia, where they range from India to the East Indies. It is common in the European Tertiary, but has not previously been known with certainty from North America. An interesting member of this flora is a large digitate species of *Oreopanax*, a modern tropical type, abundant in Central America.

The flora as a whole contains no strictly temperate elements, although many of the genera contain modern forms which range for more or less considerable distances in the temperate zone. Such a flora could scarcely flourish under existing conditions north of latitude 29°. In its general facies it is subtropical and a number of the forms indicate a high percentage of humidity, and well distributed and abundant seasonal rains, although this latter feature tends to be obscured by the large number of the inhabitants of the sandy shores which are preserved while the inland and river bank dwellers are less fully represented. A majority of the elements in this Wilcox flora could be duplicated today on the Florida Keys and the southern peninsular mainland of Florida.

Additional members of this flora not enumerated in the preceding paragraphs include representatives of the genera *Apocynophyllum, Calamopsis, Ceanothus, Celastrus, Celtis, Cordia, Diospyros, Dryophyllum, Magnolia, Malpighiastrum, Nerium, Rhamnus, Rhus, Sabal, Sapotacites,* etc., nearly all of which are new to science.

PROC. AMER. PHIL. SOC. L. 199 T, PRINTED JUNE 30, 1911.

Middle Eocene.

Middle Eocene floras are less abundant than those of the Lower Eocene since this period is marked by a considerable subsidence and deeper waters in the Mississippi Gulf, which, however, eventually

Fig. 2. Sketch map showing the approximate relation of land to water in the Middle Eocene. Stars indicate fossil plant localities, diagonal lining indicates submerged areas.

became shallower again and duplicated in a measure the Lower Eocene conditions.

At a number of localities in Georgia and at two or three in northern Mississippi and in Arkansas representatives of the Middle Eocene flora have been collected. In Georgia where the plants are associated with shallow water and estuarine invertebrates I found

the remains of a typical mangrove flora associated with types which today characterize the tropical and subtropical beach jungle. This flora includes an *Acrostichum* closely allied to the modern *Acrostichum aureum* Linné which is such an abundant fern in the mangrove and nipa tidal swamps. Other genera represented by fossil forms are *Conocarpus, Dodonæa, Ficus, Malapœnna, Pisonia, Momisia, Rhizophora, Sapindus, Terminalia,* and palms of the genus *Thrinax*. Botanists familiar with the flora of the torrid zone will recognize at once that this is a typical strand flora of the tropics which might almost have been taken bodily from Schimper's classic Indomalayan Strand Flora, or which can be seen today along the Florida Keys and in the West Indies.

The plants of this age from Mississippi and Arkansas do not indicate such a well marked ecological group nor quite such high temperatures as those from Georgia, nevertheless they also are largely subtropical coastal types and embrace species of *Sabal, Rhamnus, Panax, Ficus, Dryandroides, Persea, Sapindus,* etc. One of the most interesting forms abundantly represented in northeastern Arkansas is a citraceous form with alate petioles which I have named *Citrophyllum*. Additional genera which are present are *Nectandra* and the coniferous genus *Arthrotaxis*.

In Fig. 2 is shown the approximate position of the shore line along which the mangrove and the tropical beach flora migrated northward in the path of northerly flowing tropical ocean currents.

Upper Eocene.

No upper Eocene floras are known from the coastal plain but it is believed that future discovery will reveal their presence when the area where they are likely to occur shall have been examined in detail.

Lower Oligocene.

The Lower Oligocene has yielded no plants except petrified fragments of the wood of palms and dicotyledons. The sediments are more or less impure marine limestones, and if marginal deposits with plants were laid down they were subsequently destroyed by erosion, or have not yet been discovered.

Extensive marine faunas indicate even more torrid conditions than in the preceding epoch, uniformly distributed over this whole area.

Middle Oligocene.

The Middle Oligocene deposits are those of shallow tropical waters with a bottom temperature of at least 39° C. (70° F.), marine toward the east with true reef corals in Georgia, but becoming brackish or fresh toward the west, by reason of their shallowness and the increased volume of fresh water from the Oligocene Mississippi and Tennessee rivers and other streams. The flora is scanty but includes tropical swamp types, the fern genus *Acrostichum* being the most abundant form collected.

The accompanying sketch map (Fig. 3) shows in a generalized way the relation of land and water in the Middle and Upper Oligocene. It is to be noted that the great Mississippi Gulf had been reduced to a very wide and shallow reentrant.

Upper Oligocene.

Toward the close of the Oligocene a widespread emergence of the land was inaugurated accompanied by a slight lowering of temperatures. The floras are not abundant but are represented in western Florida and central Mississippi. They contain very abundant remains of several species of *Sabal*-like palms; the large leaves of a species of *Artocarpus* or breadfruit; leaves of figs; of the *Cinnamomum* or camphor tree; representatives of the genera *Acacia, Bumelia, Diospyros, Pisonia, Gyminda, Gleditsia, Nectandra, Sapotacites, Rhamnus, Ulmus,* etc.—the latter being the only genus which is a strictly temperate type in the modern flora, although most of the genera enumerated have representatives in the warmer parts of the temperate zone at the present time.

Miocene.

A long interval followed the close of the Oligocene, during which the coast line of southeastern North America was considerably seaward from its present position, in consequence of which deposits

FIG. 3. Sketch map showing the relation of land to water in the Middle and Upper Oligocene. Stars indicate fossil plant localities, diagonal lining indicates submerged areas.

of this age are unknown. This interval comprises the first half of the Miocene age and when renewed submergence furnishes us with a record we find very different conditions from those previously enumerated. Either because of the diversion of the gulf stream to the eastward due to the emergence of peninsular Florida or as a result of changes in depth off the Hatteras anticline, a cool inshore current seems to have swept southward along the coast and through the Suwannee Strait across northern peninsular Florida, carrying with it a northern marine fauna which replaced the tropical fauna that had previously occupied this region.

The fossil plants of this age are unfortunately rare and are as yet unknown south of the Maryland-Virginia area. The accompanying sketch map (Fig. 4) shows in a generalized way the upper Miocene conditions after the resubmergence of the area, the maximum emergence during the lower Miocene being unknown. The

FIG. 4. Sketch map showing the approximate relation of land to water in the Upper Miocene. Stars indicate fossil plant localities, diagonal lining indicates submerged areas.

land masses southeast of the mainland are to be noted as well as the supposed directions of the ocean currents.

The known fossil plants from the Atlantic coast Miocene, exclusive of diatoms, include the following species from the Maryland area near Washington described by Hollick:[2]

[2] Hollick, Md. Geol. Surv., Miocene, 1904, pp. 483–486, tf. a–b.

Quercus Lehmanni Holl.
Ulmus basicordata Holl.
Cæsalpinia ovalifolia Holl.
Rhus Milleri Holl.
Pieris scrobiculata Holl.
Phyllites sp., Holl.

In addition to the above the following have been described from the same horizon at Richmond, Va., by Berry:[8]

Salvinia formosa Heer?
Taxodium distichum miocenum Heer.
Salix Raeana Heer.
Carpinus grandis Unger.
Quercus calvertonensis Berry.
Rhus Milleri Holl.
Planera Ungeri Ettings.
Ficus richmondensis Berry.
Platanus aceroides Goeppert.
Podogonium? virginianum Berry.
Dalbergia calvertonensis Berry.
Celastrus Bruckmanni Al. Br.
Nyssa gracilis Berry.
Fraxinus richmondensis Berry.

These plants indicate that the coast was low, which explains the absence of any but the finest terrigenous materials in the shallow water deposits which constitute the Calvert formation. The flora from Virginia indicates the presence of extensive cypress swamps, the latter type of plant being the most abundant fossil collected and the other plants identified being for the most part similar in their physiological demands upon their environment. The flora from Maryland is the natural counterpart of that from Virginia in containing several typical elements of just the sort of a plant association found on sands (inner beaches and more or less stationary dunes) along the present coasts in the temperate zone.

Regarding age the plants are clearly Middle Miocene according to European standards. They indicate less conclusively the climatic

[8] Berry, *Journ. Geol.*, Vol. 17, 1909, pp. 19–30, tf. 1–11.

conditions which prevailed along the Miocene coast in this latitude. There is considerable evidence of a scant rainfall, that is to say of less than 30 inches annually but this may well have been merely a coastal condition. Indirectly the lack of land derived sediments in the deposits points to the conclusion that relatively dry conditions extended over wider areas. The mean annual temperature is difficult to determine. Several of the closely allied modern plants such as the existing bald cypress do not extend north of Maryland in the existing flora, while *Ficus* does not fruit north of Virginia, which also marks the northern limit of *Planera*. However, the Miocene forms enumerated are all different specifically from the existing members of their respective genera and the conclusion is reached that the Calvert flora would grow under the climatic conditions prevailing at the present time between Sandy Hook, N. J., and Cape Henry, Va., and that the mean annual temperature which they indicate is between 50° and 55° F.

Pliocene.

Pliocene floras have been unknown from North America until last year when deposits of this age with abundant fossil plants were discovered in southern Alabama. The most remarkable form in this flora is the fruit of *Trapa*, the water nut, which Raimann in Engler and Prantl segregates from the family Onagraceæ to form the family Hydrocaryaceæ. In the existing flora this genus has only three species of southern Europe and southeastern Asia but it is well known in the older Tertiary of North America and Europe and in the later fossil floras of Europe. Another interesting species in this Alabama Pliocene flora is a species of *Glyptostrobus*, a coniferous genus allied to our bald cypress which is now confined to eastern Asia, but which appears to have been cosmopolitan in Tertiary times. Other elements of this flora are abundant live-oaks (*Quercus*); several species of elm (*Ulmus*); abundant twigs, seeds and cone scales of a species of cypress which is very close to the existing bald cypress (*Taxodium*). Additional elements are species of *Nyssa, Hicoria, Planera, Betula, Dioscorea, Prunus, Pinus*, etc. This flora is quite modern in its facies and is a mixture of swamp

types and those of live-oak barrens. Among existing localities which I have visited which impress me as duplicating the climatic and other physical conditions indicated by this late Pliocene flora are the estuaries along the gulf coast of Alabama and western Florida, among which Apalachicola, Mobile, Perdido and Pensacola bays are the larger. The Santa Rosa peninsula which separates the latter from the Gulf of Mexico supports a flora that is very similar to this Pliocene flora and one or two of the species represented in both are closely allied and may even be identical.

Pleistocene.

Pleistocene plants are also common throughout most of the coastal plain and when they shall have been thoroughly studied they will yield a large body of exact facts which will throw much light upon the immediate ancestry and migrations of our existing flora. Already more than one hundred species have been recorded, most of which are still existing and these indicate a very different geographical distribution from that of the present coastal plain flora.

Conclusion.

I have only had time in the foregoing remarks for a very fragmentary and incomplete sketch of the present study which has really only just commenced. With the complete exploration of the area and the additional collections which it is hoped to make it is believed that the combined results of the speakers studies of the fossil floras and those of his associates on the fossil faunas and the areal geology will furnish a basis for reconstructing the physical, faunal and floral history of the southern states, during the several millions of years from the Cretaceous to the present, which will constitute a lasting contribution to the history of the earth.

AN OPTICAL PHENOMENON.

By FRANCIS E. NIPHER.

(*Read April 21, 1911.*)

In 1871 in a letter to Tyndall, Joseph LeConte gave an interesting discussion of an ocular illusion which had been previously described. Tyndall communicated it to the *Philosophical Magazine* (XLI., p. 266). The phenomenon was observed in the manner here described:

Pierce a card with a pin. Hold it before the eye at a distance of four to six inches, looking through the hole at a bright background. Place the pin in front of the eye with the head central in the pupil and in close proximity. The pin head will be "seen in the hole," and in an inverted position.

As was pointed out by LeConte, this is not an optical image but a shadow. As proof of this he cites the fact that if a series of holes are made in the card, a similar appearance of the pin head is seen in each hole. He adopted the idea that objects are seen erect, because the nerve fibers at the lowest point on the image see the top of the object in the direction along which those rays have come. He also argued that the inverted appearance of this shadow, which was erect on the retina, was in harmony with this explanation.

The well-known fact that this point in the image is the vertex of a cone of rays, whose base is the pupil of the eye, and that this diverging bundle of rays, when traced outward, does not define the position of any external point, is sufficient explanation of the fact that this line of reasoning has not been generally adopted. Evidently the fact that there are no rays has also been taken into consideration. It does not seem quite evident that nerve fibers at the lower point of the image on which ether waves collapse and deliver their impulses could "see" that these waves had their origin at a definite point, at the top of an object, at a definite distance from the refracting media, in which the radii of curvature of these waves were reversed in direction. And these waves from this point on the object are involved in a summation of waves from other and adjoining points.

Many observers have doubtless had experiences like those which the writer had years ago while doing survey work. Two transit instruments were available, one of which showed the object viewed in erect, and the other in inverted position. A few days of use of either instrument enables the observer to give proper signals to the rod-man in a perfectly automatic way. After having thus become alternately educated, an attempt to use these instruments at random for brief intervals, relying wholly on what he sees through the instrument for the information which is to guide him in making his signals, leads to the most helpless confusion. The observer even seeks to find his way out of his difficulties by comparing what he sees through the instrument with the impression received by a direct view.

Such experience as this appears to justify the conclusion that we see external objects as we have learned how to see them, by help of our other senses. Even then it is a matter of never-ending wonder that we have in our possession certain nerve-fibers that can be trained to see.

There are many interesting features of the phenomenon which LeConte discussed which appear to have escaped his attention. His claim that the sharp outline of the pin head seen in the hole could not be an optical image, since such an image would be so much out of focus as to be invisible, is justified to this extent. The object is in fact also visible in its real position in shadowy outline. It appears transparent, and the inverted shadow of the pin head is mentally projected outward and appears to be visible through the object itself. Every detail of the letters on a printed page is visible through this enlarged and transparent appearance which the object itself presents, due to an out-of-focus image on the retina.

The sharpness of outline of the shadow decreases as the hole is made greater in area. This is due to penumbral effects. A black card gives more sharply defined results than a white one. A tube having the pierced card at one end and the pin head at the other may be applied to the eye, in such a way as to cut off all side light. The head may be covered with a black cloth, which is also wrapped around the tube. The shadows are then as sharply defined as an optical image could be. If the black sateen cloth be thrown over the head, and the eyes be directed towards a bright sky, a multitude of cir-

cular images like pin hole images will be seen between the crossed fibers. Some of these are due to the right eye and some to the left. A pin head in front of either eye will show multitudes of inverted pin head shadows.

A circular disk of white paper having a diameter of 1 mm. or slightly less, mounted upon a black card will also have upon it a sharply defined black shadow of the pin head, if the side facing the observer is illuminated. The paper disk must be near enough to the eye so that its image on the retina is out of focus, as in other cases where the pin hole is used. At various points on the glowing end of a cigar, when observation is made in a darkened room, similar shadows may be observed. A small blot of ink on a sheet of white paper will yield a white shadow of the pin head. The same result is given by a hole in a white card, if the card is illuminated and observation is made through the hole at a dark background.

If the reflected image of the full moon or of a bright star from the convex surface of a lens be used instead of the pin hole in a card, the inverted shadow will be observed. If the reflecting surface is concave, the shadow will appear erect if the eye is placed between the reflector and its principal focus. If the eye and pin are in the divergent beam beyond the principal focus, the shadow of the pin head will appear inverted.

It is evident that when the shadow on the retina is erect, it appears inverted, and *vice versa*.

The eye lens and retina may be replaced by a convex lens and a paper screen upon which an image of the moon may be cast. A pin closely in front of the lens will show no shadow. If another convex lens be now placed in front of the lens representing the eye, the moon's image will be out of focus. The moon's image may be in front of or behind the screen, according to the position of the second lens. The shadow of the pin will then appear.

The capacity for accommodation of this artificial eye is unlimited, and the second lens may be dispensed with. The screen being placed between the lens and the image, the shadow of the pin will appear erect on the screen. When placed beyond the image, it will appear inverted.

If an opera glass be focused on a street lamp 50 meters away a

pin head between the eye and the eye-lens will produce no shadow on the retina. If the glass be focused for a nearer object, an erect shadow will appear. If focused for a more distant object, the shadow will appear inverted. A hole through a card and with a bright background may be viewed by means of the opera glass. The hole may have any diameter from 0.05 to 1.5 cm. The distance of the card must be adapted to the diameter of the hole, and may vary from close contact with the object lens to three or four meters, the glass being focused for a more distant object. The results are as indicated above. The setting sun surrounded by bright clouds may be used as an object, if viewed through the foliage of trees thirty or forty meters distant, the glass being focused for an object more distant than the trees. The mass of foliage will be dotted with pin head shadows. Each opening through the leaves acts in a manner similar to the pin hole.

In all of the cases described, the shadow upon the retina is by some mental act projected outward in space. An interesting question arises concerning its apparent position. LeConte says that in his experiments it appears in the hole in the card. Perhaps it would be proper to say that it is seen through the hole. The hole itself may have a diameter of about one third that of the pin head, and the pin head then appears smaller than the hole. Its apparent size depends somewhat on the diameter of the hole.

If a pin is placed back of the card and in erect position so that it is visible through the hole, it may be so placed that it has the same apparent size as the shadow. If the pin is at a distance of 30 cm. from the eye, and the card is at a distance of 15 cm., the shadow and the pin will have the same apparent size. The appearance of the inverted shadow and the erect pin is as shown in Fig. 1.

FIG. 1.

This suggests an interesting device whereby the line of sight of the two eyes and the capacity for muscular adjustment may be exam-

ined. Pierce a card with two pin holes, at such a distance from each other that when placed at half the distance of distinct vision from the eyes, they may be seen as one. This can be done by drawing lines across the ruled lines of a page of white paper, and crossing the ruled lines symmetrically so that at the top of the page the lines are farther apart and at the bottom they are nearer together than the two eyes. Pierce pin holes at each intersection of the ruled lines with the cross lines. If held in front of the eyes so that the cross lines are seen double, the two inner images of the lines will appear to cross. At this distance apart thus determined two holes will appear as one. Place a card having holes thus placed in front of the eyes. Mount two pins in front of the pupils so that the two shadows appear superposed in the superposed images of the holes. Two pins may now be placed back of the card so that when viewed through the holes they will also appear superposed. The two holes and the four pins will then present the appearance shown in Fig. 1. This arrangement locates two points along the line of sight of each eye. The holes may be in separate cards which close the ends of two tubes, through which the observations are made. These tubes, together with the pins, should be capable of screw adjustments.

When the pin hole is viewed through a tube which is lined with dark paper, the card serving to close the outer end of the tube, it may be used for an examination of certain imperfections in the eye. For example, in my own case one eye shows a minute hole with a bright background to be of uniform appearance. Viewed by the other eye a rather sharply defined shadow is shown in the center of the hole. This is due to a slight irregularity in the curvature of the outer surface of the cornea. This is due to a grain of gunpowder which was blown into the eye from a horse-pistol which was discharged from a distance of about 35 cm., into the lower part of the face, about fifty years ago. The grain of powder was visible for many years, but has been gradually absorbed. A slight distortion of closely ruled parallel lines indicates that an irregularity of the surface still persists. The shadow seen in the pin hole shows that light is not uniformly spread over the retina when a slightly divergent beam of light enters the pupil. Any opacity in the crystalline lens would also produce a shadow upon the retina.